

Thinking In Images: A Philosophical Re-Assessment

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I hereby declare that the herein thesis has been
composed by me and that it is my own work.

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Abstract

This thesis addresses the issue of the nature of thought - specifically, the issue of whether or not it is ever correct to say that people 'think in images'. It is argued that people do make use of image-type cognitive representations when they are engaged in certain kinds of spatial problem solving and that they perform 'mental rotations' with these image representations. This thesis is developed in six chapters.

Chapter one provides an introduction to the mental imagery research of psychologist Roger Shepard and his associates. Three experiments designed to test for the image rotation phenomenon as Shepard defines it are set out and discussed in detail.

Three distinct theses of interpretation of the results of the image rotation experiments are to be found in the current literature of cognitive psychology and philosophy of psychology. They are the 'quantification thesis', the 'causal thesis' and the 'metaphysical thesis'. These theses are discussed in Chapter two. It is argued that the quantification and the causal theses of interpretation are warranted by the evidence adduced in Shepard's experiments, but the metaphysical thesis or interpretation is not.

Chapter three provides an account of the metaphysical status of the phenomenon of image rotation. It is argued that image rotations are brain states that people experience. A computer model for image rotation is set out and the methodological implications of conceiving image rotations as experienced brain states are discussed. In particular, it is argued that introspections of image rotations have an

observational status and ought, accordingly, to be taken quite literally and seriously by cognitive investigators.

Chapter four contains a consideration of four linguistic strategies for spatial problem solving. These strategies have been proposed by psychologists and by philosopher, Daniel Dennett, in order to discredit the theory of mental rotation. It is argued that the strategies are, at best, incapable of producing the linear reaction times exhibited by Shepard's subjects and, at worst, such that they make impossible the actual solution of the spatial problem.

In Chapter five, a consideration of the long-standing philosophical difficulties with imagistic thought is given. Four basic difficulties are identified. It is argued that the idea that images are a special kind of representation that display information in an analogue form and that image rotation is an analogue process is not patently ridiculous, and in fact can be developed in a coherent way that violates neither philosophical nor empirical considerations.

In the conclusion, Chapter six, there is a discussion of the possible role of imagery in a theory of cognition. It is argued that while it may not be the case that images are the basic elements of thought, they may still be cognitively significant.

Introduction: An Old Problem Resurfaces

During the past fifteen years, the topic of mental imagery has captured a considerable amount of experimental and theoretical attention. This has been a period of excitement, of discovery, of bold hypotheses and of competing theoretical interpretations. The experimental work in question implicates mental imagery not only as an empirical phenomenon of considerable predictive importance, but also, at the theoretical level, as a major representational system underlying human cognitive behaviour. Especially in North America, this research is hailed by some as reflecting a change in the direction of experimental psychology, and a reaction against the excesses of the behaviourist tradition.

The most striking of the imagery experiments to date have to do with visual imagery. These experiments employ a variety of methodologies ranging from the straightforward solicitation of verbal reports from subjects, in a manner reminiscent of the early introspectionists, to the application of sophisticated techniques of mental chronometry, involving the measurement of subjects' reaction times when they are solving spatial problems. The data generated in these experiments have emboldened psychologists to make a number of very specific claims about the visual image. It is claimed, for instance, that visual images can be generated more or less at will, that they can be scanned for new information; and that the smaller the image is, the harder it is for the subject to glean details from it. More interesting and more controversial perhaps, are claims which focus on the possibilities for image manipulation. Roger Shepard and his colleagues have argued, on the

basis of a series of experiments conducted at Stanford University, that people can perform mental rotations with their images and that these rotations proceed in a manner analogous to the actual physical rotation of real objects. That is, the rotations are continuous and take place at a measurable angular velocity.

Of course, few if any of these 'new' findings in psychology will be surprising to most people. That we have visual imagery and can manipulate it much as we might manipulate real objects is uncontroversial--a simple fact of our mental lives. Our ordinary conversation is replete with references to such experience. We talk of a 'mind's eye' and of 'seeing' or 'visualizing' images, pictures, and objects before the mind's eye. We claim, as well, to be able to solve problems, particularly to conduct spatial calculations, by generating and manipulating these images.

If we want to know how many windows grandmother's house has, we simply call up an image of the house and count the windows as they are thereby presented. An engineer engaged in designing a machine component, an automobile mechanic taking an engine apart, a plastic surgeon planning the re-setting of a facial bone, a new homeowner considering various possible arrangements of livingroom furniture, a student of elementary geometry, who is told by his teacher to bisect an angle, and a native New Yorker giving directions from the city centre to the south-bound freeway--all these people whose thoughts are concerned with the spatial relation of things, with their shapes and their colours--proceed, so they think, imagistically. And most of them would claim to be completely at a loss in these activities if the power of imaging were suddenly to leave them.

Most philosophers, on the other hand, will have no truck with mental images or their kind, and so they are bound to find the psychologists' excitement over the new imagery experiments surprising and even, perhaps, somewhat annoying. They are likely to remind us that Wittgenstein and Ryle laid imagery definitively to rest some years ago and that, in any case, the arguments against imagery are a priori and as such incontrovertible on empirical ground. So whatever science may happen to discover in its cognitive investigations, it cannot discover that people think (even sometimes) in images.

The truth of the matter, philosophers will insist, is that mental images are nothing but fictions - metaphysical ghosts generated out of the Cartesian view of the dualistic nature of man. Belief in such entities was supportable when the requirements of logic and science were less stringent than today. But to regard imaging as if it were a process that could take place somewhere 'in the mind' and to talk of images as if they had some necessary connection to thought, is to reaffirm a demonstrably futile model of human conduct. And it is bound to distract attention from what we are really about. Our business as thinkers, philosophers argue, is to talk understandingly to ourselves or to others, in private or in public.

So, to hear the philosophers tell it, there is nothing more to be said about or done with mental images. Our best bet as professionals would be to put them behind us--exorcising them altogether from current thinking. Philosopher Daniel Dennett espouses this policy explicitly: "To be able to dispose of mental images," writes Dennett, "would be a

clear case of good riddance."¹

The measure of the philosophers' conviction in this regard is the fact that they have remained for the most part silent in the face of this resurgence of interest in imagery among their colleagues in psychology. To my knowledge, only three philosophers in the mainstream of philosophical psychology--Daniel Dennett, Owen Flanagan, and Jerry Fodor have seen fit to make comment on the imagery experiments and, of these, only Fodor does this with any degree of seriousness. Dennett, for instance, purports to 'deal with' Roger Shepard's imagery experiments in the space of a mere one and three-quarter pages of his most recent work in the philosophy of psychology. And Flanagan mentions these experiments in the course of a discussion of methodology in cognitive science, but forbears any commentary on the possible consequences of this work for the philosophical treatment of thought in general or mental imagery in particular. Apparently, Flanagan thinks there are no consequences.

It might be supposed that philosophers generally have not had the opportunity to become acquainted with the extent of the imagery research phenomenon or with the precise nature of the claims that are thereby generated. But this is hardly likely. The number of articles reporting new research findings and the number of commentaries (both critical and laudatory) on these, appearing in reputable journals of psychology, has increased exponentially over the past ten years. There is even a new journal, begun in 1980, called the Journal of Mental Imagery. So

¹Daniel Dennett, Content and Consciousness (London: Routledge and Kegan Paul, 1969), p. 132.

it seems entirely reasonable to expect that philosophers--and especially those who work in the field of philosophical psychology--would have had more than adequate opportunity to take note of this literature in the course of their research.

No, the explanation for this silence appears to be that philosophers do not think that they have need to join the imagery debate once again. The question is, is this silence acceptable? Does the a priori character of the philosophical position regarding mental images and their possible relation to thought, make redundant any further commentary? Surely it does not. The data generated in the imagery experiments are compelling. They point rather directly to the cognitive significance of mental imagery and this stands in stark contrast to the philosophers' staunch refusal to countenance the possibility of imagistic thought. The philosophers may well be confident that these data present no threat to their current theories and beliefs about thought that, indeed, mental images are so far beyond the pale that nothing could resurrect them, but it is surely incumbent on them to say so, and to explain just why this is so. Ex cathedra statements that imagery must be one way or another can serve no purpose here; bald assertions do not constitute arguments or explanations. A successful theory of thought must be able to provide an account of the 'facts' of thought. And the data generated in the imagery experiments are now part of these facts. Somehow philosophers must find a way to explain them and if they cannot then they must look to the adequacy of their own theories.

Broadly speaking, this thesis may be described as an attempt to fill in this missing piece in the philosophy of psychology--first and foremost to draw attention to the imagery research, and secondly, to

supply the much-needed treatment of it. The focus of attention here will be quite specific--the mental image rotation experiments of psychologist, Roger Shepard. These experiments are of considerable intrinsic interest but, more importantly, they are, in my judgment, the best and clearest examples of the image research phenomenon to be found in the experimental literature. Ultimately, I hope to provide a modest defence of mental imagery on the strength of the evidence adduced in Shepard's experiments. I shall be arguing that certain kinds of spatial cognition demand imagistic explanation, and that the philosophers' refusal to countenance it is nothing less than prejudicial.

CHAPTER I

Tracking Mental Rotations

1.1 The Phenomenon of Mental Rotation

Students of the human mind have long noted its ability to mimic, internally, the possible motions and transformations of objects in the external world. In his Enquiries of 1748, philosopher David Hume wrote that to "join incongruous shapes and appearances costs the imagination no more trouble than to conceive the most natural and familiar objects," and that "this creative power of the mind amounts to no more than the faculty of compounding, transposing, augmenting, or diminishing the material afforded us by the senses and experience."¹ Similarly, a century later, German physicist and sensory physiologist, Hermann von Helmholtz observed that "memory images of purely sensory impressions ... may be used as elements of thought combinations without it being necessary, or even possible, to describe these in words," and further, that "equipped with the awareness of the physical form of an object, we can clearly imagine all of the perspective images which we may expect upon viewing from this or that side, and we are immediately disturbed when such an image does not correspond to our expectations."² And Albert

¹D. Hume, Enquiries Concerning Human Understanding and Concerning the Principles of Morals, L.A. Selby-Bigge (Ed.) (Oxford, 1951), Section II.

²R.M. Warren and R.P. Warren, Helmholtz on Perception: Its Physiology and Development (New York: Wiley, 1968), pp. 252-54.

Einstein stated that he "very rarely" thought in words at all. Indeed, he maintained that his "particular ability" did not lie in mathematical calculation, but rather in "visualizing ... effects, consequences and possibilities." He could not even undertake the difficult business of finding words and mathematical symbols to communicate his new insights into the nature of space and time, he said, until he had already worked out his conceptualization of the physical situation by means of a "more or less clear image which can be voluntarily reproduced and combined."¹

Such observations are intriguing. Indeed, one is inclined to wonder just how genuine this purported mimicry of operations performed with objects 'in the mind' of operations performed with objects in the physical world really is. Can we do mentally what we might do physically?

Suppose an individual is asked to compare two objects - a pear, say, and a tennis ball - and decide which is the larger. Certainly this is easily done if representatives of these objects are physically present. The individual simply places them side-by-side and observes them. But can this comparison take place otherwise? Can the objects be compared a priori - in the mind? Suppose that an individual is making decisions about possible arrangements of office furniture. There is a large desk which the individual in question would like to situate in a corner of the office, but it is not clear that the desk will fit. Once again, the simple and direct solution would involve a physical

¹Albert Einstein, Autobiographical Notes, in P.A. Schlipp (Ed.), Albert Einstein: Philosopher-Scientist (Evanston, Ill.: Library of Living Philosophers, 1949).

intervention of some sort - the desk might be moved into the corner or, less strenuously, the individual could undertake to make a rapid spatial calculation with the help of a tape measure or ruler. But is it possible that the requisite spatial calculation could be achieved without physical intervention of any sort? Could the individual move the desk into the corner in his mind and 'see' whether or not it will fit?

If the Hume-Helmholtz-Einstein introspections are essentially correct, if memory images can be used as "elements of thought," and if there is a faculty for "compounding, transposing, augmenting and diminishing" these images, then there would seem to be no problem. Images of a pear and a tennis ball respectively could be called up or generated from memory and placed into juxtaposition and the desk could be mentally moved into the corner. But how will we know for sure--casual introspections in themselves seem only to be suggestive?

The course of research undertaken by Roger Shepard and his associates on the general topic of 'mental images and their transformations' bears on this question. The research indicates that there may well be something to this idea of mimicry postulated by Hume, Helmholtz, and Einstein, at least in respect of one kind of mental operation. This is the operation which Shepard describes as 'mental rotation'.

What is mental rotation? To get an intuitive feeling for it, and for the kind of experiments that Shepard and his colleagues have conducted, consider the three pairs of line drawings of geometric shapes which are presented in Figure 1 below. Now decide as quickly as possible whether or not the objects in the pairs are a match for one another.

Figure 1

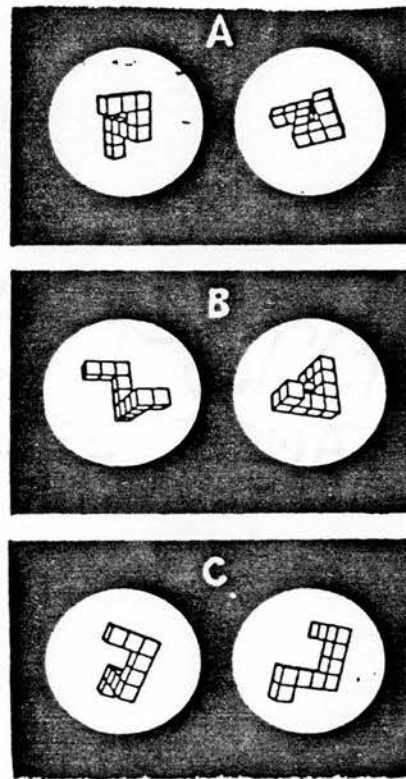


Figure 1 is taken from Roger Shepard and Lynn Cooper, Mental Images and Their Transformations (Cambridge, Mass.: MIT Press, 1982), p. 35.

How did you arrive at a solution for the problem presented in A? Did you perform the calculation descriptively - counting, perhaps, the number of blocks in each shape and noting the number, location and direction of each of the right angle turns? Perhaps you performed the calculation by a rotation of one or the other of the shapes into a position of congruence with its counterpart. Or perhaps, for the sake of rigour, you sought to combine the descriptive and rotational strategies -

counting the number of blocks and then, when you had concluded that there was an equivalence (at least of the number of component parts), rotating one shape in your mind into congruence with the other? Are the B pair a match? They are a match but how did you discover this? Did you not find that it was necessary to perform a rotation of one or the other shape in the third dimension, perpendicular to the page? The C pair do not match at all. Did you not discover this fact by attempting a number of rotations, none of which was successful?

As a first approximation, it might be said that mental rotation is what one might attempt to perform in the face of a problem situation (such as that presented above) which requires the assessment of the shape similarity of figures/objects not manually accessible. There are two points to be noted about mental rotation so defined.

First and foremost, it appears to be a fairly common and compelling experience. It has been reported by more than one famous scientist. For example, the German chemist Friedrich A. Kekulé said that many of his early insights into the nature of chemical bonds and of molecular structure arose out of idle reveries in which he spontaneously experienced kinetic visual images of the dancing atoms hooking up to form chainlike molecules. (As is well known Kekulé's cultivation of this visionary practice culminated in his celebrated dream in which one of these snake-like writhing chains suddenly twisted into a closed loop as if seizing its own tail; thus was Kekulé provided with his long-sought answer to the problem of the structure of benzene.¹) And in more

¹A. Findlay, A Hundred Years of Chemistry (London: Duckworth, 1948).

mundane circumstances, it is a fact that when required in an experimental circumstance, to perform an 'in principle' or 'mental' completion of an already partially constructed jigsaw puzzle, most people report using the following calculation strategy: They begin by identifying individual puzzle pieces which seem to have the right combination of bumps and grooves to be a possible match for a clearly defined space in the puzzle. Then if a given piece is out of alignment with that space, they simply rotate an imaginary representation of it in their minds until they can see whether or not an alignment of the respective bumps and grooves is possible. Similarly, when asked if frogs have lips and if they have stubby green tails, most subjects report first looking at the mouth of an imaginary frog, then mentally rotating the image, and then 'zooming in' (to use the subjects' own words) on the rear in order to have a closer look before answering.¹ And, finally, Shepard's own imagery experiments provide further documentation of the generality of the rotation experience. After the completion of a particular sequence of experimental trials involving shape assessments of pairs of geometric figures, Shepard's subjects have been invited to comment on how they came to solve the problems. The overwhelming majority of them report performing what they invariably describe as "mental rotation".²

¹These two rather unusual experiments have been conducted by Stephen M. Kosslyn and are reported in his book Image and Mind (Cambridge, Mass.: Harvard University Press, 1980), p. 1 ff.

²There have been a series of such experiments generating these results. Listed in chronological order they are: R. Shepard and J. Metzler, "Mental Rotations of three-dimensional Objects," Science, 1971, Vol. 171, pp. 701-703; L. Cooper and R. Shepard, "Chronometric Studies of the Rotations of Mental Images," in W.G. Chase, ed., Visual Information Processing (New York: Academic Press, 1973); L. Cooper and R. Shepard, "The Time Required to Prepare for a Rotated

The second point to be noted is that mental rotation appears - phenomenally - to take place very much in the manner of a rotation which might have been performed physically/manually with the puzzle pieces or with models of the geometric shapes arranged on pivots. That is to say, it has the very property of mimicry that Hume and Helmholtz describe. Subjects report that when they are solving the test problems, the process undertaken mentally is an exact replica of the process which might have been undertaken physically had it been practicable. They do with their minds what they might have done with their hands.

This suggests one or two interesting possibilities. Most basically, it suggests that there may well be some sort of correspondence to be acknowledged between - very broadly - 'what goes on' when mental images are undergoing spatial transformations of the rotational kind and 'what goes on' when the same transformations occur physically. It may be, in fact, that the processes of image rotation are not entirely 'free'. They may be governed by certain inherent constraints - constraints, say, which are roughly analogous to those which govern like processes in the three-dimensional world. Physical rotations take place holistically - in a continuous, unbroken sequence. If, for instance, a given figure is rotated 60° , that figure must cover all the distance between 0° and 60° . And proportionate to the distance factor are the factors of speed and effort. The greater the distance covered in the

Stimulus," Memory and Cognition, 1973, Vol. 1, pp. 246-250; and L. Cooper and R. Shepard, "Mental Transformations in the Identification of Left and Right Hands," Journal of Experimental Psychology, 1975, Vol. 104, pp. 48-56; L. Cooper and R. Shepard, "Transformations on Representations of Objects in Space," in E.C. Carterette and M.P. Friedman, (Eds.), Handbook of Perception (New York: Academic Press, 1978).

rotation, the longer it must take to complete the rotation (all things being equal) and the greater effort must be expended. These rules of rotation per se may well apply in the case of mental rotations.

At the beginning stages of an empirical science an assumption of regularity - of governance by certain principles or rules can be a large step forward. This is particularly so if this assumed regularity is predicated on something the researcher knows a fair bit about - such as the rotation of objects in the physical world. Shepard has converted this assumption into a very specific hypothesis regarding one kind of mental processing. The hypothesis is this: If the subjects' introspections are correct, if they perform rotations in order to make the relevant shape assessments of problematic test figures, then there ought to be a consistent and systematic correlation between the degree or distance of the rotation and the length of time taken to solve the problem. Stated in these terms, the hypothesis would seem to lend itself quite readily to empirical investigation. All that is required is a way of measuring the subjects' performance times or reaction times in relation to the distance factor.

1.2 The Mental Rotation Experiments

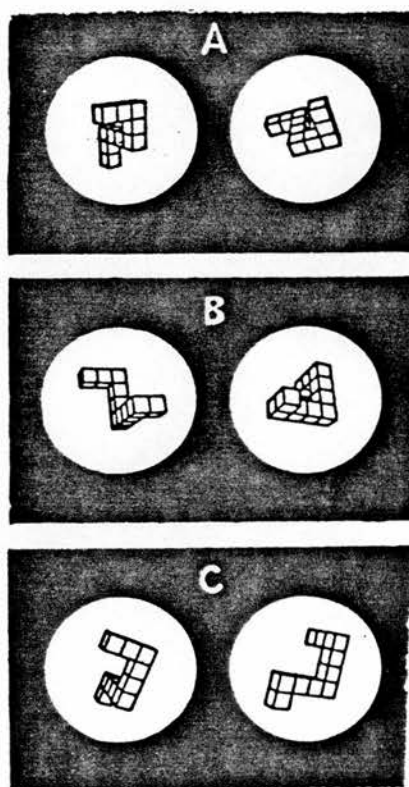
Since 1971, Shepard and his colleagues have conducted a considerable number of experiments designed to test the rotation hypothesis. The three experiments described here are the best and clearest examples of this research. In order to facilitate identification, I have provided names for each of these experiments. In order of their treatment they are: the Original Rotation Experiment, the Direction of Rotation

Experiment, and the Controlled Rotation Experiment.

The Original Rotation Experiment

In the original mental rotation experiment conducted by Shepard and Metzler in 1971, subjects saw pairs of line drawings like those presented earlier and reproduced here in Figure 2. They looked at 1600 such pairs, which were constructed according to three different rules, corresponding to A, B, and C in Figure 2.

Figure 2



- A. Here the two drawings are identical except that they have been pasted on the page at different angles. It would also be correct to say that the two drawings show the result of a rotation on the plane defined by the surface of the page.
- B. These two drawings are two-dimensional perspective projections of the same three-dimensional structure, that is, the two drawings show the result of rotation of the same object in the third dimension.
- C. Though the two drawings look similar, they are different. They cannot be rotated either on the plane of the page or in the third dimension to be brought into exact congruence.

The subjects' task with each pair of drawings, which appeared on a screen while their respective heads were kept still,¹ was to indicate, by pulling left or right hand levers, whether the depicted objects were the 'same' in the sense of A and B above, or 'different' in the sense of C. Stimulus pairs of types A and B were presented in pairs differing by anything from 20° to a maximum of 180°. With a bit of practice, the subjects averaged more than 95 percent correct responses, even though they were instructed to respond as rapidly as possible while retaining

¹That the researchers found it necessary to restrict head movement is, perhaps, telling in itself. Shepard and Metzler discovered early on in the experiments that when they did not so restrict the subjects, a number of them tried to solve the problems by 'rotating' their heads - to the side so as to see the test objects from the desired reoriented perspective. It may be that such 'head rotation' counts as an intermediate strategy between actual physical rotation of the objects themselves and mental rotation. In any case, this behaviour tends to underscore the natural and spontaneous character of the rotation strategy.

accuracy. The main datum of the experiment, however, was the speed of response, not the accuracy.

When questioned after the fact of the experiments as to the strategy they employed, the subjects all indicated that they performed mental rotations in order to solve the test problems. Significantly, the data would seem to bear them out. If mental rotation really occurred, we would expect that the larger the distance covered by the rotation, the longer it would take the subjects to decide that the drawings are the same or different. Figure 3 shows the average reaction times in relation to the angle of rotation for types A and B. For both types, the reaction time rose from about one second to more than four seconds as the rotation covered the range from 0° to 180° and it will be noted that the reaction time - angle of rotation data fall along a straight line.

Such linear dependencies have now been obtained with a variety of stimuli, including perspective drawings of three-dimensional objects composed of cubes,¹ two-dimensional random polygons,² and alphanumeric characters.³ The linearity provides strong evidence for an additive process in which the time required to go from one orientation, A, to

¹R. Shepard and J. Metzler, 1971, op. cit., and J. Metzler and R. Shepard, "Transformational Studies of the Internal Representation of Three-dimensional Objects," in R.L. Solso, ed., Theories of Cognitive Psychology (Potomac, Md.: Lawrence Erlbaum Asso., 1974).

²L. Cooper, "Mental Rotation of Random two-dimensional shapes," Cognitive Psychology, Vol. 7, 1975, pp. 20-43.

³L. Cooper and R. Shepard, "Chronometric Studies of the Rotation of Mental Images," in W.G. Chase, (Ed.), Visual Information Processing (New York: Academic Press, 1973).

Figure 3

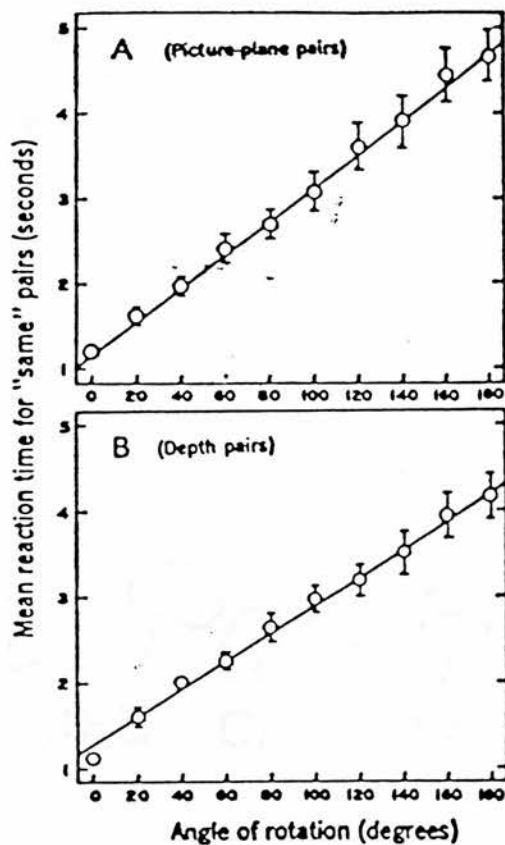


Figure 3 is taken from Shepard and Cooper, 1982, op. cit., p. 50.

another orientation, C, is the sum of the time required to go from A to an intermediate orientation, B, and the time required to go from that intermediate orientation, B, to C. It is consistent with the more specific proposal that the subjects make the determination of sameness of shape by carrying out some sort of internal analogue of an external rotation of the one object into congruence with the other, and, further, can perform this analogue process no faster than some limiting rate. In point of fact, for the eight subjects participating

in the experiment, the average rate of mental rotation was about 60° per second, or a rate of six seconds to complete a 360° rotation. The subjects said they could not rotate the images any quicker without losing track of the basic structural integrity of the images.

The other point to be noted here is that the two straight lines of Figure 3 have approximately the same slope, which means (if we accept, for the sake of argument, the rotation hypothesis) that the subjects rotated the mental images at the same rate on the plane of the page or in the third dimension. In point of fact, rotation in the third dimension would be far more complex geometrically because it would involve changes in perspective and relative size, and yet the subjects appeared to be able to handle both tasks with equal speed and facility.

It is worth emphasizing at this point, that the data would seem to warrant a spatial interpretation rather than a literally pictorial one. People blind from birth can solve the identical shape assessment problems by analyzing three-dimensional models of the geometric shapes tactilely. They, too, introspect an abstract process of spatial rotation, but, clearly, in their case, the mode of representation cannot be visual or pictorial.¹ Thus, there is no need to think that the subjects

¹This fact has been established through the studies of blind subjects conducted by P.A. Carpenter and P. Eisenberg (published in "Mental Rotation and Frames of Reference in Blind and Sighted Individuals," *Perception and Psychophysics*, 1978, Vol. 23, pp. 117-124) and by G.S. Marmor and L.A. Zaback (published in "Mental Rotation by the Blind: Does Mental Rotation Require Visual Images," *Cognitive Psychology*, 1975, Vol. 7, pp. 548-559). It was found that the reaction times of these subjects increased linearly with the angular difference between the orientations of the objects, clearly suggesting that mental rotation of some kind - very likely a kinaesthetic kind - was used to make the calculation.

rotate actual pictures but merely they are doing something more spatial than linguistic. Why else would the reaction times vary with the degree of rotation?

The Direction of Rotation Experiment

A further experiment conducted by Metzler and Shepard in 1974¹ attempts to track the direction of subjects' mental rotations. In this experiment, subjects were presented with geometric shape pairs in a sequence designed so that, if the subjects were carrying out a mental rotation, they might go the long way around the circle. More specifically, for all except a few pairs presented in the last half of the experimental sequence, a particular (i.e., clockwise) rotation would carry the object on the left into the orientation of the object on the right by the shortest rotation (i.e., the rotation of no more than 180°). Then, on a few randomly inserted trials toward the end, the direction of shortest rotation was unexpectedly reversed so that, if the subjects were continuing to rotate in the same direction as before (as they would have been more or less conditioned to do), they would end up going the long way around. In order to minimize the likelihood of their discovering that they were going the long way around on these special trials, the pairs were confined to just the next step beyond 180° which, in view of the 45° increments used in this experiment, was 225° (as opposed to 135° , if they happened to reverse direction of rotation on that trial).

¹J. Metzler and R. Shepard, 1974, op. cit.

These special trials could provide crucial information concerning the nature of the internal process by means of which the subjects determined sameness or difference of the two objects on each trial. For according to the mental rotation hypothesis, the internal process is in a sense a simulation of an external process of rotation, and, as such, has a definite trajectory and a definite direction along that trajectory. Thus, to the extent that subjects always go the long way around on these special trials, their reaction times should be even greater than at 180° and, indeed, should coincide with the linear extrapolation to 225° of the reaction-time function already obtained for angular departures from 0° to 180° . Or, to the extent that subjects sometimes go the long way around and sometimes reverse direction to take advantage of the 135° shortcut, their reaction times should be bimodally distributed with an upper mode corresponding to the linear extrapolation to 225° and a lower mode corresponding to the reaction time previously found for 135° . In contrast to this, a nonrotational theory would seem to predict no such directional effect. The controlling variable would then be simply the absolute difference between the two pictures and, hence, the reaction time should be the same no matter what the possibilities for direction of rotation may be.

The experiment results confirmed Shepard and Metzler's expectations - the subjects' reaction times were indeed bimodal. For pairs which differed either by 225° if the subjects rotated in the previously prevailing direction or by 135° if the subjects rotated in the opposite but shorter direction, there were two distinct peaks, one aligned with the mode for the standard 135° pairs and the other centered on a linear extrapolation of the reaction-time function past 180° to 225° .

Apparently, the subjects sometimes carried out their mental rotations in the usual direction even though that took them the long way around, and sometimes they noticed the reversal and so went the short way around. This, in any case, was what the subjects themselves reported when asked to discuss their particular rotational strategies. In fact, by explicitly instructing the subjects as to the direction in which the mental rotation was to be carried out, Lynn Cooper, in a subsequent experiment, obtained reaction times that increased with remarkable linearity, all the way out to 300°. ¹

The Controlled Rotation Experiment

A more complex task was devised by Cooper and Shepard² in which the subjects were required to judge whether common alphanumeric characters were presented in their normal form or as mirror image reversals. During each trial, the subjects sat fixating a blank circular field projected in a tachistoscope. To start a new trial, the experimenter orally announced which of two characters, an "R" or a "2" (with which the subjects had been previously familiarized), was scheduled to appear as the test stimulus on that trial. Then the experimenter started playing a tape on which the verbal commands "up," "tip," "tip," "down," "tip," "tip" had previously been recorded at a controlled rate of one command per half second. On the basis of prior instructions and

¹L. Cooper, 1975, op. cit.

²L. Cooper and R. Shepard, 1973, op. cit.

practice trials, the subjects were to imagine the normal version of the announced character starting in its upright orientation and rotating clockwise at a rate of 60° per half second, in synchrony with the auditory commands. (Thus the initial command "up" notified the subjects to begin rotating the internally generated mental image from its initial upright orientation and, three commands later, the word "down" indicated that this image should now be rotated around into its 180° orientation.) To assist the subjects to keep pace with the auditory commands, there were six small tick marks visible around the border of the circular field at 60° steps (starting at the centre of the top. See Figure 4.).

Figure 4

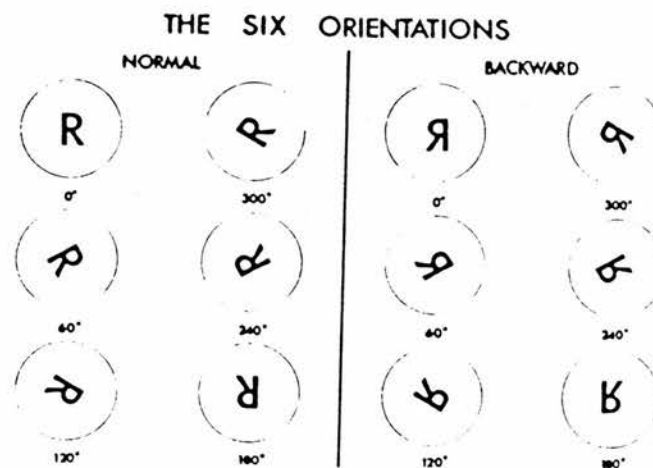


Figure 4 is taken from Shepard and Cooper, 1982, op. cit., p. 77.

At a random point during this purely mental process, some version of the imagined character - either in the orientation that the subjects, in accordance with the verbal instructions, should be imagining at that particular moment in this rotation, or in some other orientation chosen at random from among the six possible 60° increments - was displayed. If the subjects were actually carrying out a mental rotation, Cooper and Shepard reasoned, then the speed with which they can discriminate whether the probe stimulus is normal or backward (i.e., mirror reversed) should be greatest when the probe appears in the orientation momentarily assumed by their mental image, for only then will they be able to make an immediate match between their internally rotating image or template and the externally presented test probe. Equally, it ought to be the case that the speed of the subjects' discriminations should decrease in proportion to the degree of the orientation discrepancy between the anticipated orientation and the actual orientation of the presented test probe.

However, the theoretically critical point of this experiment resides not in the proposed linearity itself but in the indication it can give that the underlying process is composed of parts (corresponding to rotations through smaller angles) that are necessarily performed in sequential order and for which, consequently, the performance times are additive. Thus no matter what the effective rotation times may be between particular adjacent points separated by 60° around the circle, if the time required to go from any one point to any other nonadjacent point is an additive combination of the component times to go between the intervening adjacent points, then the average time to go between points in all pairs separated by X 60° steps should increase linearly

with X. To show that the time to rotate from A to C is an additive combination of the times to rotate from A to B and from B to C is to furnish another kind of evidence that the process of rotating from A to C passes through a point, B, corresponding to an intermediate orientation. A finding of linearity would thus support further Shepard and Metzler's original supposition that mental rotation is an analogue process.

The experimental results confirmed the expectations of the experimenters. The subjects were able to classify a test stimulus as normal (as opposed to backward) in only 500 to 600 msec., as long as the orientation of that stimulus coincided with the rotating orientation expected. This tends to support Cooper and Shepard's belief that the subjects arrived at their classification by matching the presented probe against a "rotating" mental image. And, as predicted, when the probe appeared in some orientation other than the (rotating) orientation expected, reaction time increased markedly with the difference between the expected and the actual presented orientations. Indeed as Figure 5 attests, the overall increase, from 0° to 180° is close to 400 msec., making each 60° increment roughly 130 msec. in time to complete. This second experimental result appears to make good sense only if we are prepared to assume that when the orientation of the test probe fails to agree with the imagined orientation, the subjects undertake an additional poststimulus rotation in order to achieve a match between that probe and the internal representation of the corresponding normal character and that, when this occurs, this poststimulus rotation passes through whatever intervening orientations there may be. As in the case of a physical rotation of the character 'R' around a circle, the mental rotation of an imagined 'R' must pass through the 120° orientation if it is to

move from 60° - 180° .

Figure 5

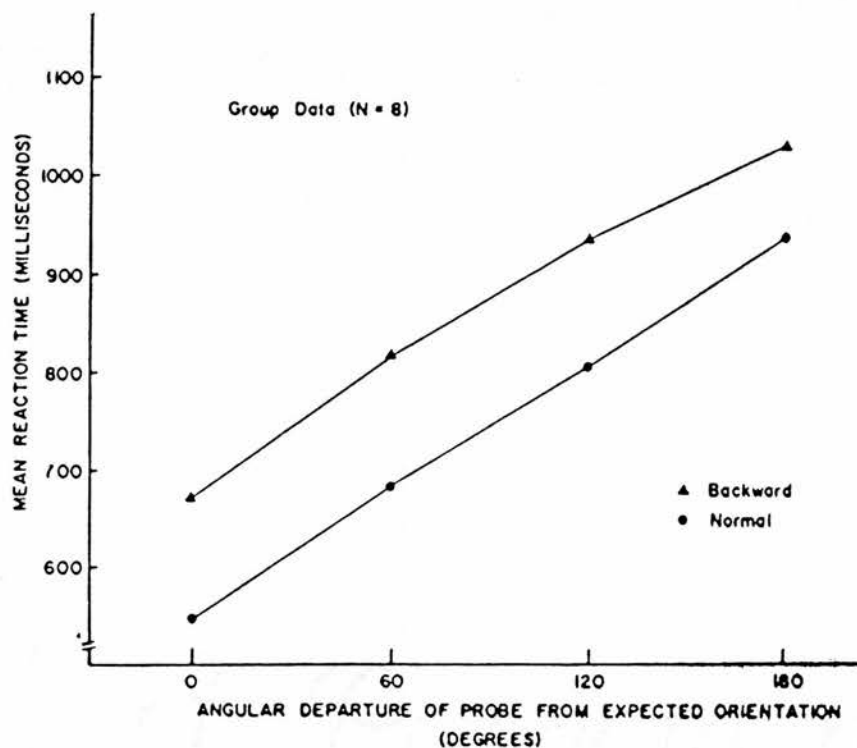


Figure 5 is taken from Shepard and Cooper, 1982, op. cit., p. 108.

Once again in accordance with the standard operating practice undertaken in all of the rotation experiments, Cooper and Shepard requested the subjects to state how they came to solve the problem. Cooper and Shepard describe their findings: "Our subjects typically claim that in preparing for the anticipated presentation of a rotated stimulus, they did in fact a) form a mental picture of the anticipated

stimulus and then b) carry out a mental rotation of that picture into its anticipated orientation."¹

Taken together, the rotation experiments establish four hitherto unknown facts about the problem solving behaviour of human beings relative to the given set of spatial problems. These facts are as follows:

- i) The time required for people to complete the shape assessment process varies consistently and linearly with the degree of orientation disparity displayed by the figures/objects under consideration. In point of fact, it takes the individuals tested just twice as long to assess objects displayed at an orientation disparity of 100° as it takes them to assess objects displayed at an orientation disparity of 50° --just the kind^{of} temporal proportionality that could be expected if people were either making their shape assessments on the basis of actually having to wait out object pairs being physically rotated into a congruence or 'fit' with one another or doing something comparable mentally.
- ii) Reaction times are bimodally distributed for angles beyond 180° --in correspondence with the individual's choice of going the long or the short way around the 360° circle.
- iii) The shape assessment process can be completed in advance of the presentation of a second stimulus--provided that the individuals tested already know the axis and direction of the required rotation.
- iv) introspections of mental rotation are consistently correlated with these reaction-time patterns. When questioned after their performance in any of the rotation experiments described above, individuals report mental rotation.

¹Roger Shepard and Lynn Cooper, 1982, *ibid.*, p. 74.

The principal claim that Roger Shepard has been prepared to make on the basis of these studies of mental rotation is this: in the course assessing the global shapes of two similar-appearing objects that differ appreciably in orientation, the individuals tested necessarily pass through a series of internal states that bear a one-to-one relation to the physical states that the object would pass through if it were physically rotated from ^{one} orientation to the other. Shepard writes:

... by measuring reaction times to variously oriented test stimuli presented during the course of a mental rotation, we have established that the intermediate states of the internal process do indeed have a one-to-one correspondence to intermediate orientations of the external object. Our results, in fact, show that there is actually something rotating during the course of a mental rotation--namely, the orientation in which the corresponding external stimulus, if it were to be presented, would be most rapidly discriminated from other possible stimuli.¹

And, commenting on the Controlled Rotation experiment, Shepard and Lynn Cooper write:

Mental rotation is an analog process with a serial structure bearing a one-to-one relationship to the corresponding physical rotation.... In mentally rotating an object between any two widely separated orientations, the internal process passes through the mental image corresponding to the external object in some intermediate orientation. Consequently, the orientation at which the subject is most prepared for the appearance of that object at each moment is actually rotating with respect to the external world.²

¹Roger Shepard, "The Mental Image," American Psychologist, February 1978, p. 135.

²Roger Shepard and Lynn Cooper, 1982, op. cit., p. 118.

1.3 Theoretical Significance

In the context of the general subject of human cognitive processes, the study of a particular, apparently nonverbal, non-logical process of problem solving such as mental rotation of three-dimensional figures or alphanumeric characters, may seem rather specialized and, perhaps, of little or no consequence. After all, Shepard and his research teams have not demonstrated through the rotation experiments that thought in general is accomplished in images.¹ Nor has Shepard demonstrated that the many familiar objects that people encounter in various positions in their day-to-day dealings with the world, have to be mentally rotated into some canonical orientation before they can be recognized. On the contrary, the truth of the matter is probably that in the case of most objects and symbols with which people have to deal, there are sufficiently numerous, redundant, or orientationally invariant distinctive features that people are well able to achieve recognition directly--without the need of a preliminary mental transformation.

What can the significance of the rotation experiments be then? Do they license any inferences about the nature of thinking/problem solving? Are there lessons for cognitive scientists to learn from them?

¹Indeed it is important to note that, in the literature, Shepard has been described as a 'dual-coding' theorist. This simply means that it is a general assumption of Shepard's theory of cognitive representation that the human brain has available to it two distinct symbolic systems for representing and processing information. Roughly, it can be said that one system is specialized for dealing with linguistic, sequentially organized information and for generating speech while the other system is specialized for representing and processing information in a direct, analogue fashion. A second assumption of Shepard's theory is that these two systems can, at times, be functionally interdependent such that during the course of a given problem solving episode, both systems may be called into play.

A demonstration that human subjects are capable of mentally rotating spatially structured objects even if only occasionally and in certain rather contrived circumstances, would be of considerable theoretical significance. Most basically, such a demonstration would call into question the advisability of formulating theories of human cognition solely in terms of discrete processes of language manipulation and logical analysis as has been characteristic of much recent psychology and philosophy. At some (yet to be specified) stage in its cognitive operations, the human brain, apparently, displays analogue capabilities and properties and, at this stage, its activity cannot adequately be described in terms of the manipulation of language. Accordingly, philosophers and psychologists would have to acknowledge once and for all, that people can and, indeed do, do more by way of thinking than just talk quietly to themselves--describing objects and events and making inferences on the basis of these descriptions.

Equally, a confirmation of the rotation hypothesis would raise a question about the advisability of taking too glibly or too literally any comparison which might be made between the human brain and a digital computer. A digital computer may calculate the coordinates of a rotated structure by performing a matrix multiplication. But the intermediate states of this row-into-column calculation, are nothing like those which would characterize a mental rotation. The intermediate internal states of the latter (and, by extension, of any analogue process) would have a natural one-to-one correspondence to appropriate intermediate states in the external world. The intermediate states of a logical computation would not in general display such correspondence. At the very least, then, if we say that the brain is like a digital computer, we must add

the qualification that it is like a digital computer with analogue simulation capabilities.

Moreover, if Shepard is correct in his 'analogue' assessment of the problem solving activities undertaken by the individuals tested in the rotation experiments, then it is not unreasonable to expect that the imagining, understanding, and planning of many other kinds of spatial operations may also be accomplished in an analogue manner. We may well, to hark back to the examples cited earlier on, be able to mentally move the desk into the corner and assess its size relative to the available space and we may well be able to walk around our respective grandmothers' houses 'in our minds' and count the number of windows there.

CHAPTER 2

Interpreting the Mental Rotation Experiments

2.1 Introduction

As might be expected, the rotation experiments have not gone unnoticed by the community of cognitive psychologists. Indeed, the experiments have generated a good deal of commentary. Much of this commentary has been very positive. For example, psychologist Alan Paivio has argued that the rotation experiments provide an 'objective measurement' of the mental activity of Shepard's subjects--a way of 'tracing' or 'quantifying' the process they introspect and describe as 'rotation'. Writes Paivio:

Roger Shepard's recent work on the rotation of mental images has involved completely objective reaction-time methods which not only confirm the subjective impression that we rotate mental images, but also provide a means of measuring the speed of such rotation.¹

No less positively, Fred Attneave argues that the findings of the original rotation experiment of 1971, cannot be explained in anything less than analogue terms. He writes:

¹Alan Paivio, "Images Propositions and Knowledge," in Images Perception and Knowledge, J.M. Nicholas (Ed.), (Boston: D. Reidel Publishing Co., 1974), p. 50.

... the most compelling evidence I know for the existence of a tridimensional modeling medium in the head comes from the work of Roger Shepard and his students. Shepard and Metzler's experiments of 1971 ... show beyond any reasonable doubt that when one rotates a mental image from one aspect to another the representation of the object is in fact going through all of the intermediate aspects in a continuous manner. I have no idea how anybody could possibly account for these results without postulating an analogue representational medium.¹

And Roger Brown and Richard J. Herrnstein argue that the reaction-time findings vindicate the common sense belief in a 'mind's eye', with all, presumably, that this entails in terms of a metaphysics of mind. Write Brown and Herrnstein:

The upshot was that there is an icon which is somewhat analogous to the physical stimulus itself. In an older vocabulary, based on personal experience rather than experimentation, we might have said that the data suggest a mental image of the physical stimulus. Common sense has always believed in a "mind's eye" in addition to the physical events in our sense organs. In that respect, the research has just confirmed what people already know.²

There are, it will be noted, really three distinct theses of interpretation being proposed here. The thesis in the first instance makes an assertion about what grounds or evidence might establish the presence of mental rotation activity and about how it is possible to quantify or

¹Fred Attneave, "How Do You Know," American Psychologist, 1974, p. 498.

²Roger Brown and Richard J. Herrnstein, "Icons and Images," in Ned Block, (Ed.), Imagery (Cambridge, Mass.: The MIT Press, 1981), p. 29.

measure this activity. For Paivio, the positive dimension of the discovery of the linear reaction-time behaviour pattern, is that it makes possible an empirical investigation of a phenomenon hitherto considered to be simply out of the reach--beyond the pale for science per se. For identification purposes, I call this the 'quantification thesis'. The thesis in the second instance, is straightforwardly causal. Attneave is asserting that the explanation for the problem solving behaviour of Shepard's subjects is analogue activity. To put the thesis bluntly, the subjects could not solve the test problem in the way that they do unless they undertook some such activity. The thesis in the third instance--certainly the boldest of the three--is a metaphysical thesis. Brown and Herrnstein apparently believe that the reaction-time results have implications for the truth of some form of metaphysical dualism. They argue that the results prove that there is a 'mind's eye' (as plain common sense has always believed) and that this mind's eye and its activity are separate and distinct from 'physical' events.

In this chapter, I want to consider each of these theses of interpretation in turn and to decide whether any or all of them are warranted by the results of the rotation experiments.

2.2 The Quantification Thesis

Certainly this is the most popular interpretation of the rotation experiments to be found in the literature. In addition to Alan Paivio, psychologist Stephen Kosslyn, James P. Pomerantz, Steven Pinker, George Smith, and Steven P. Schwartz have seen fit to propose versions of it.

In articles written jointly by Kosslyn and Pomerantz¹ and by Kosslyn, Pinker, Smith and Schwartz,² Roger Shepard and his research team are praised for proving the 'reality' of the reported rotation activity and for developing, by way of the rotation experiments, a technique which facilitates a kind of 'quantification' of this activity. Kosslyn and Pomerantz exemplify this attitude toward the research when they write, "The quantification of introspection technique provides not only more reliable data than that obtained via simple introspective reports, but more fine-grained information as well. Thus not only did these sorts of findings begin to tell us something about imagery, but they seemed to lend credence to the claim that images are indeed psychologically real entities, that they are worthy of being studied."³ W.G. Chase makes very similar comments on the image rotation research. He writes, "The importance of this research is that it develops an objective way of measuring one of the mental operations commonly used in visual thinking."⁴ And in the introduction to the volume of essays devoted to the discussion of current findings in cognitive psychology, W.K. Estes

¹Stephen Kosslyn and James R. Pomerantz, "Imagery, Propositions and the Form of Internal Representations," Cognitive Psychology, vol. 9, 1977, pp. 52-76.

²Stephen Kosslyn, Steven Pinker, George E. Smith, and Steven P. Shwartz, "On the Demystification of Mental Imagery," The Behavioural and Brain Sciences, vol. 2, 1979, pp. 535-581.

³Stephen Kosslyn and James Pomerantz, "Imagery, Propositions and the Form of Internal Representations," op. cit., p. 57.

⁴W.G. Chase, "Elements of Information Processing," in Handbook of Learning and Cognitive Processes, vol. 5 of the series Human Information Processing, W.K. Estes (Ed.) (New Jersey: Lawrence Erlbaum Associates, 1978), p. 78.

describes the 'timing' technique, used by Shepard and several other cognitive psychologists in the following terms: "I refer of course to the use of reaction times to trace mental processes. The idea of ascertaining the durations of unobservable mental processes by subtracting reaction times for initiation of earlier from those of later stages of the task ... has evolved into an extremely sophisticated technique."¹

Now, clearly, the upshot of each of the commentaries cited above is that the reaction time constitutes a kind of objective parameter of what is going on 'inside'--because of it, we can be sure that mental rotation is, as Kosslyn and Pomerantz say, 'psychologically real' and we can even, as Estes claims, trace the duration of the rotation activity.² Furthermore, the reaction time is to be preferred over the verbal report as a behavioural indicant of rotation activity as it has, according to Kosslyn and Pomerantz, the virtue of reliability. There is always the possibility when a verbal report of image rotation is made that the reporter in question is telling a deliberate lie or that he simply does not appreciate the ordinary language conventions for talk of 'rotations'--either of images or of objects and so, presumably, has misidentified-misdescribed his problem solving activity. These possibilities, presumably, do not arise with the reaction time. Reaction times which increase or decrease in precise proportion to the degree of

¹W.K. Estes, Introduction to Handbook of Learning and Cognitive Processes, Vol. 5, Human Information Processing, *ibid.*, p. 2.

²Presumably, what Estes has in mind here is that the image experience begins upon the posing of the experimental test question and ends when the subject responds to the question. Its duration, therefore, is whatever period has elapsed between those two objective performances.

orientation disparity of the test stimuli could not be faked. Thus the appeal of substituting the reaction-time pattern as the decisive sign or 'test' of mental rotation for the verbal report is clear--it makes possible a proper scientific inquiry into a phenomenon of mind.

I should think that this interpretation of the rotation experiments would have some fairly considerable appeal within the experimental psychology community. After all, in science it is common to make phenomena that allow of precise measurement into the evidence or grounds which decide the application of a referring expression. A term that was previously applied on qualitative grounds--on grounds of introspection--becomes determined by quantitative grounds, once quantitatively measurable correlations are established. The adoption of the 'rapid eye movement' test for dreams is a striking example of this. In the late 1950's, psychologists William Dement and Nathaniel Kleitman conducted an experiment which established a significant correlation between the presence of the REM (Rapid Eye Movement) and the reporting of dream experience. The procedure in the Dement and Kleitman experiment was to waken subjects from sleep during periods of REM and also during periods when there was no REM, in order to find out whether or not they would report dreams. The incidence of dream report was high after the REM awakenings (152 out of 191) and very low after the non-REM awakenings (11 out of 160). Dement and Kleitman drew the following conclusion from their findings.

It is reasonable to conclude that an objective measurement of dreaming may be accomplished by recording REM's during sleep. This stands in marked contrast to the forgetting, distortion, and other factors that are

involved in the reliance on the subjective recall of dreams. It thus becomes possible to objectively study the effect on dreaming of environmental changes, psychological stress, drug administration, and a variety of other factors and influences.¹

I do not claim to know whether or not Dement and Kleitman actually made the decision, based on this experiment, to use eye movements as their test for the occurrence of dreams. It is clear, however, from the comments given above that they were strongly drawn to that decision, and this is quite understandable. After all, they wanted to do scientific work on dreams, and therefore needed a reliable method of determining with precision when dreaming occurs and exactly how long it lasts. And this need, so they thought, is not filled by individual subjects' 'subjective reports' of dreams. But whether or not Dement and Kleitman themselves adopted REM as the test for dreaming, it is pretty clear that over the course of time and of subsequent experiments and discussions regarding dreams, the scientific community did so embrace it. We are told, for instance, that whether or not on a given night we recall having any dream experiences, that we did indeed dream. Statistics resulting from subsequent REM experiments indicate that during a normal night's sleep, people exhibit the REM pattern off and on all night long. And, more strikingly, in the case of individuals who claim that they never dream or dream (so they say) 'only once in a blue moon', we are told that they are just wrong about this. They are dreaming--

¹William Dement and Nathaniel Kleitman, "The Relation of Rapid Eye Movements During Sleep to Dream Activity: An Objective Method for the Study of Dreaming," Journal of Experimental Psychology, Vol. 53, 1957, p. 341.

which is to say, they exhibit the REM behaviour, they just do not remember their dreams.

Indeed the acceptance of the REM as the decisive test for the occurrence of dreams among psychologists has become so standard and uncontroversial that it is simply stated as a matter of fact in introductory textbooks. Discussing the results of the original Dement and Kleitman experiments in 1957 and the experiments conducted in 1958 by Dement and Wolpert in which an attempt was made to time the REM episodes, the author of one such text says this:

These findings indicate that the average adult dreams about 1.5 hours every night--the time spent in REM sleep. How can we square this assertion with the fact that in everyday life, most people seem to experience dreams only occasionally and that some deny that they ever dream? The answer is that dreams are generally forgotten within minutes after they have occurred.¹

Similarly, David Kretch, Richard S. Crutchfield and Norman Livson, authors of another standard text write:

This objective approach to dream measurement has made it possible to show that all individuals dream very frequently, perhaps four or five times a night. Furthermore, it has been found that most of this dreaming is not remembered. When subjects are awakened as soon as ten minutes after REM's stop, very few of the dreams are reported.²

¹Henry Gleitman, Psychology (New York: W.W. Norton and Company, 1981), p. 354.

²David Kretch, Richard S. Crutchfield and Norman Livson, Elements of Psychology (Toronto, Alfred A. Knopf Ltd., 1969), p. 764.

To throw light on the question of whether those who report fewer dreams are forgetting them or actually dreaming less, an investigation was carried out by J.S. Antrobus, W. Dement, and C. Fisher.¹ These investigators recruited two groups of subjects--those who recalled very many dreams and those who recalled almost none. These groupings not only were based upon a self-report as a 'dreamer' or 'non-dreamer', but also were further checked by requiring the candidates for those groups to keep 'dream diaries' in which each morning, any recalled dreams were recorded. The final groups of eleven subjects each were therefore quite extreme, representing the two poles of dream recall.

Records of the frequency of REM's were then obtained for each subject. The findings were that the 'non-dreamers' spent virtually as much of their sleep time in REM activity as did the 'dreamers'--19 percent versus 23 percent. And, significantly, both groups exhibited the same number of periods of REM activity over the course of the night--the non-dreamers had an average of four such periods and so did the dreamers. Apparently, the percentages in dream time differed because the dream recallers had longer, but not more frequent, dreams. A portion, but only a small one, of the difference in dream recall between the two groups may thus have to do with the fact that the dream recallers had slightly more dream material to recall. "But most of the discrepancy in dream recall," Antrobus, Dement, and Fisher have argued, "must be attributed to some sort of forgetting mechanism that operates sometime

¹J.S. Antrobus, W. Dement, and C. Fisher, "Patterns of Dreaming and Dream Recall," Journal of Abnormal Social Psychology, vol. 69, 1964, pp. 341-44.

between the occurrence of the dream and the opportunity to report it."¹

It is interesting to note that the REM test for dreams has a wider currency and acceptance than simply within the confines of the scientific/professional community per se. Information regarding the REM research has, over time, filtered down into the ordinary language context, and it has had quite a dramatic effect on most people, especially those who routinely keep abreast of the scientific research which finds its way into the popular press. One finds, in fact, that when questioned about their dreams the night before, people will often say--"I didn't dream at all--to tell the truth, I slept like a baby." And then with an almost palpable degree of self-consciousness, add the correction--"I guess I did dream, I just wasn't aware of it," or "I just forgot it."²

Now it seems to me that the scientific/professional and, more recently, lay interpretation of the Dement and Kleitman experiments on dreams constitutes a kind of paradigm case or model of the interpretation of Shepard which is under discussion here. And the analogy to what might be described as the 'reaction-time case' for mental rotation activity is strong. In the case of both experiments, the dream experiment and the mental rotation experiment, a body of 'objective'

¹Antrobus, Dement, and Fisher, *ibid.*, p. 343.

²For what it is worth, I have engaged in a discussion of dreams with a class of introductory philosophy students and I have found both that they are well-aware of the REM research and that they are quite willing to surrender their 'dream report autonomy' to the received authorities. In fact, several students volunteered the information that people dream during sleep even though, upon waking, they cannot recall having done so. The 'proof' that this is the case, they say, is the fact that people--all people--exhibit REM activity.

data has been adduced. Both REM's and reaction times are publicly observable and scientifically quantifiable--in fact, highly sensitive instruments of measurement are employed for the purpose of tracking the REM and the reaction time, respectively. And in the case of both of these experiments, a significant empirical correlation has been established between the relevant behaviour pattern and the report of the corresponding experience. According to Dement and Kleitman's findings, when subjects are awakened during REM sleep, they almost always report dream experiences. Likewise, (and indeed even more impressively) Shepard's subjects, when they are questioned after exhibiting the appropriate reaction-time behaviour, consistently report mental rotation activity.

So there would seem to be very good reasons--reasons of utility and reasons of convention, for adopting the quantification interpretation of the rotation experiments. Are these reasons decisive then? Let us put the reaction-time test to work in a concrete setting. Suppose that we follow the example set by psychologists and physiologists in their treatment of dreams and make the reaction-time pattern the test of mental rotation activity. This means, then, that we know when a person has undertaken mental rotation in relation to a shape assessment problem by the way that he behaves during the course of an experimental session. If the subject displays the specified reaction-time behaviour pattern--if, that is to say, his reaction times are a function of the degree of angular disparity exhibited by the test objects, then we know that he has mentally rotated an image of one or the other of the objects in the presented pairs independently of any claim that he might make in this regard. And we know for how long he has been engaged

in this activity. Conversely, if the subject does not display the reaction-time pattern--if, that is to say, his reaction times are disproportionate to the angular disparity of the test objects, or the reaction times are a constant value, then we know that he has not been engaged in mental rotation activity but has undertaken some other kind of problem solving strategy--very probably a linguistic/inferential one.

Presumably, then, the application of the reaction-time test would be as simple and mechanical as that. No longer would we have to rely on the subjects' truthfulness, or take for granted that their understanding of what a mental image and what mental rotation is, is not idiosyncratic such that their verbal report while quite possibly sincere, is just beside the point because they are not using the expressions 'mental image', 'mental rotation' in accordance with ordinary language rules for them. And no longer would we have to ask the subject for his impression of the length of the rotation exercise--whether it was of long or short duration for instance--we can actually time this by the use of a mental chronometer.

So far so good, but suppose a different scenario. Suppose that a given subject displays the appropriate reaction-time pattern, but when questioned as to his experience, claims that he did not 'see' in his mind's eye, images rotating into congruence. He says, in fact, "I don't know what was going on, my mind was a blank. I just solved the problems, that's all." And when he is primed to report mental rotation by being asked, "Did you not rotate images in your head?" he says, "No." Are we entitled to tell him that he is wrong about this--that he did 'see' images of the test objects rotating, but that, for some reason, like the dreamer, he does not recall it?

Clearly, this is precisely what we are committed to if we follow through our intention to use the reaction-time behaviour pattern in the manner of the REM--that is as the decisive test for the image rotation activity. And this, presumably, is what Paivio, Kosslyn and Pomerantz and the other psychologists who advocate the adoption of the quantification interpretation of Shepard's work must have had in mind all along--the simple, efficient, even mechanical application of an objective test for the identification and localization of a subjective state/process.

At first sight, this overruling of the subject's verbal report may seem unduly authoritarian. But consider the alternative. If we say that dissenting subject 'X' did not rotate images even though he exhibits the linear reaction-time pattern--then we are obliged to explain just why this linearity should come about at all. And why should it, if not by way of some kind of rotation activity? An alternative, non-analogue problem solving strategy based on the generation of linguistic descriptions of the test objects, would seem to predict relatively flat reaction times and certainly not the strict linearity that subject 'X', in this instance, exhibits. In order to explain this linearity, then, we must deliberately set out to invent a novel hypothesis. And candidate possibilities for this hypothesis do not readily present themselves.

Now, let us suppose the reverse situation to be the case. This time, the subject in question does not display the linear reaction-time behaviour pattern--but claims at the end of the experimental session, that he saw images of the test objects rotate into positions of mutual congruence. Do we tell him that he is mistaken--that his memory, in

this instance, has simply failed him?¹

The rotation experiments establish that there is a significant (and hitherto unchallenged) correlation between subjects' introspections of mental rotation and reaction-time behaviour of a very particular kind. If there is an isolated instance where this correlation does not occur, in particular, where the introspection is not coupled with the appropriate behaviour, then we have a good reason to doubt the accuracy of the original introspection. Indeed, we may conclude that while the subject may well be using mental images in some unspecified way, he cannot have undertaken a mental rotation, strictly speaking, otherwise his reaction times would vary precisely in accordance with the number of degrees that one of the test objects in the presented pairs must be rotated in order to achieve a congruence or fit with the other.²

¹As I have already indicated, Shepard's data regarding verbal reports of calculation strategies, do not provide any empirical support for either of these suppositions. The fact is that all of Shepard's subjects display the linear reaction times and none of them have failed to report mental rotation during the problem solving sequences. This fact notwithstanding, it is still theoretically possible that some individual, on some occasion, may make the relevant shape assessments, exhibit the reaction-time pattern, and yet fail to report mental rotation, or, vice versa, the subjects may fail to exhibit the reaction-time pattern, but report mental rotation. Consideration of such possibilities, however unlikely, are, I believe, instructive.

²Preliminary results (for only three subjects) of an experiment carried out at Stanford by psychologist Arthur Thomas provide further support for this. In the Thomas experiment, subjects were trained to encode test stimuli (of roughly the type used in Shepard's original rotation experiments) descriptively, and to judge the sameness or difference of the pairs in terms of a match or mismatch of the generated descriptions. A consequence of this coaching was that the subjects' reaction-time functions became very nearly flat. Indeed, for one of the subjects, the reaction time at 180° was only about 150 milliseconds longer than at 0°, rather than some three to four full seconds longer as in the original 'uncoached', rotation experiments. That this is so clearly indicates that the recalcitrant subject's report of mental

I would suggest, then, that just as people frequently forget whether and what they have been dreaming, they may well forget whether and how they solve problems and that all things considered, this 'lapse of memory' hypothesis best explains the recalcitrant cases in question here. Normally, of course, when subjects fail to report mental rotation activity upon questioning, we ought to take it that there has been no such activity. It is only when these reports are coupled with linear reaction times that we have reason to overrule them. Equally, it is reasonable to assume that mental rotation activity is the normal cause of verbal reports of mental rotation. But if such reports are coupled with non-linear reaction times, then, once again, we have reason to conclude that despite what a given subject may say, he did not engage in mental rotation activity. These are simply untypical cases brought about by a failure of memory. Almost always people remember mental rotation activity, but sometimes (at least hypothetically) they do not.

It is worthy of note that the 'lapse of memory' hypothesis regarding problem solving strategy is supported by independent experimental evidence. When asked to add a series of six numbers scattered at random about the surface of a page, adult subjects do so very quickly and, most often, correctly. But when asked to specify in what sequence they actually performed the addition, most subjects falter and finally come to admit that they do not know. Clearly the subjects knew at the time of the actual addition, and, if required, they could have performed the

rotation is mistaken. Flat reaction times appear to be the result of an alternative, non-rotational strategy. (The results of this experiment are unpublished. They are reported, however, in R. Shepard and L. Cooper, 1982, *ibid.*, p. 65.)

calculation out loud, indicating, thereby, the precise order of the numbers. But, somehow, in the brief period of time that elapsed between the actual addition and the question, they have simply forgotten. Such forgetting seems to be equally possible in the case of mental rotation-type problem solving.¹

Can we expect, then, that the reaction-time test will be acceptable to the lay public? Will people be prepared to have their own introspective judgments regarding the presence or absence of mental rotation problem solving, gainsaid on the basis of contradictory reaction-time evidence? In principle, I see no reason why not. If people can come to adopt a convention regarding the evidential connection between REM's and dreams, then they can come to adopt a convention regarding the evidential connection between reaction times and mental rotation. Specifically, the convention to be adopted here is that reaction-time patterns decisively establish the presence or absence of mental rotation activity and they establish the temporal duration of this activity.

All that is required for this acceptance to come about is the education of the public to the facts of the rotation experiments over time. Should such education take place, we might expect to find individuals asserting, matter-of-factly, that (just as they know they dream several times every night, even though, more often than not, they do not recall having done so) they know that they rotate images in order to solve shape assessment problems involving pairs of objects. And some may

¹This experiment was conducted by Saul Sternberg in 1966. It is reported in "High Speed Scanning in Human Memory," Science, 213, pp. 501-4.

even say that they know this even though, so far as they can recall, they have never actually 'seen' images rotating before their mind's eyes. They will be assuming, of course, that, in this, they are like everyone else. The scientific evidence indicates that people solve a particular kind of problem by mental rotation. They infer (quite reasonably) that they do likewise.¹

So the reaction-time test for mental rotation, could, like the REM test for dreams, gradually filter down from a strictly scientific context into the ordinary language context. That there could conceivably develop a convention around the reaction-time test invites an intriguing speculation. Could the reaction-time test not become a criterion (of mental rotation) in Wittgenstein's sense? Frankly, I do not think so. It is one thing to establish the occurrence of a given mental process (and this, I take it, the reaction-time test clearly does) but quite another to actually define that mental process. According to Wittgenstein, it is the latter task, which a criterion must perform. Strictly speaking, 'X' is not a criterion for "Y" if people could learn the meaning of Y without having grasped the connection between X and Y. And, surely, this can be accomplished in the case of

¹Practically speaking, of course, though people may talk about the reaction-time test, they will never be able to use it to identify instances of the occurrence of mental rotation activity. A linear reaction time is not, like pain behaviour, observable with the naked eye. We need special instruments of measurement in order to detect such minimal gradations in reaction time from one case to another. This is the one respect in which the analogy between the reaction-time test for mental rotation and the REM test for dreams breaks down. Apparently, we could observe REM unaided by special equipment-though in the normal course of things, we would rarely, if ever, do so.

mental rotation. The truth of the matter is that even if, as per our hypothetical scenario, the reaction-time test became an accepted convention of ordinary language, people could (and arguably, would) still come to know what mental rotation is, they could (and would) still develop a considerable facility for mental rotation talk, by hearing what other people say about mental rotation and observing in what context they say this.¹ In talking about mental rotation, people might add, of course, that linear reaction times always go along with it, but this is in no sense crucial, it does not tell us what mental rotation is. It is simply a helpful extra piece of information about a way that it is possible to identify mental rotation activity in others.

What it means to engage in mental rotation activity is defined by what people generally report of such activity. Verbal reports are the criteria of mental rotation. Linear reaction times are strictly speaking, effects or, to use the appropriate Wittgensteinian language, 'symptoms' of the process of mental rotation. The evidential value of linear reaction times relative to mental rotation is taught by experience--scientists have come to discover that linear reaction times

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Mental rotation talk could arise, for instance, in an art gallery where a particular abstract piece is being considered. Someone might say, "...imagine what this would look like if it were tilted on one side, or turned upside down." And the response comes, "... Yes, I can see it, rotated a little bit, it looks like a such-and-such, rotated even more, it looks like something else." Or mental rotation talk might arise in a more straightforward problem solving context where, as in the formal experiments, spatial quantities, proportionalities are being assessed.

coincide with verbal reports of mental rotation.¹

There is a final, perhaps parenthetical, point that I would like to make in connection with the discussion of criteria. As is well-known, in the book Dreaming, Norman Malcolm argues forcefully against the notion that REM's could possibly count as criteria for dreams. Obviously, for precisely the same reason that I believe that linear reaction-time functions cannot be criterial for mental rotation, I would agree with Malcolm--REM's are not criterial for dreams. Malcolm's justification for this position is interesting. As far as he is concerned, the verbal report is the last word on dreaming. It not only establishes the meaning of dreaming--defines what it is to have a dream--but it also functions as the decisive test for the occurrence of episodes. According to Malcolm, we know whether or not an individual has been dreaming by what he says. If he says he has not been dreaming, then the issue is

¹I realize, of course, that various--frequently opposing--interpretations of Wittgensteinian language theory abound in the literature and that there has been considerable debate over exactly what Wittgenstein meant by the notion of 'criterion'. I do not intend to adjudicate this debate as it is beyond the scope of the present inquiry. The interpretation of criterion that I have utilized is to be found in P.M.S. Hacker (Insight and Illusion: Wittgenstein on Philosophy and the Metaphysics of Experience, London: Oxford University Press, 1972) and in Anthony Kenny, (Wittgenstein, Harmondsworth: Penguin Books, 1973). It seems to me that this interpretation accurately reflects the Wittgenstein position (at least) as it is developed in the Philosophical Investigations. It is particularly reflective of this passage: "When I say the A B C to myself what is the criterion of my doing the same as anyone else who silently repeats it to himself? It might be found that the same thing took place in my larynx and in his ... But then did we learn the use of the words: "to say such-and-such to oneself" by someone's pointing to a process in the larynx or the brain." (L. Wittgenstein, Philosophical Investigations. London: Oxford University Press, 1953, Section 376.)

settled--we must take his word for it. No other empirical evidence is relevant to the case. Writes Malcolm:

But if someone tells a dream or says he had one he is not making a 'subjective' report which may or may not agree with 'objectivity reality'. His waking impression is what establishes that he had a dream, and his account of his dream establishes what the content of his dream was. If he had a vague impression of his dream then it was a vague dream. If he is not certain whether he dreamt then there is uncertainty in reality. His impression is the criterion of reality and therefore it cannot be characterized as subjective. Subjective and objective are one in the case of dreams.¹

According to the theory I have been expounding, Malcolm is dead wrong about this. It is quite true that REM is not criterial for dreaming, it is also true that the verbal report is criterial; even so, the actual occurrence of dreaming can be and (as it turns out) most often ought to be established on REM evidence. Clearly, the source of Malcolm's position is his belief that if something is criterial for something else, it is also the decisive test for it. This is just not the case. Verbal reports of dreaming can be a test, but they are not the only test, nor, more significantly, are they the decisive test. If subject 'X' says he has not dreamt at all during the night and four periods of REM activity have been recorded he is wrong. The REM establishes this decisively.

¹Norman Malcolm, Dreaming (London: Routledge and Kegan Paul, 1959), p. 80.



2.3 The Causal Thesis

Simply stated, the causal thesis is this: The problem solving activity of the subjects participating in the rotation experiments is to be explained in terms of an analogue process whereby images (or 'analogue representations') of the problematic test objects are generated and rotated into positions of mutual congruence or non-congruence, as the case may be. As compared to the quantification thesis, the causal thesis makes the stronger claim. For, clearly, it is one thing to say, (as Paivio, and Kosslyn, Pomerantz and company do) that linear reaction-time patterns enable us to identify the occurrence of mental rotation activity, and enable us to measure the duration of that activity, but it is quite another to say that the occurrence of such patterns prove that rotation activity is actually functional--that, as a matter of fact, people solve spatial problems of the requisite type in this way.

This stronger thesis is favoured by Shepard himself (as the passages cited at the end of Chapter 1 indicate), by Fred Attneave (see the passage on page 33), and by Alan Paivio. In Paivio's case, of course, the thesis is proffered as a companion to the quantification thesis.

Paivio writes:

Shepard and his collaborators have been able to measure the speed of such mental rotations using reaction time methods. (This is the quantification thesis) ... The significance of these experiments for the problem of knowledge is that they provide experimental evidence that we know how things appear directly, in a visual sense, as well as how they appear when they have been transformed in some way. The results tell us that the mental representations must contain a dynamic component that functions

internally much as perceptual-motor processes do when applied to the concrete world of objects and events.¹

Not surprisingly, it is the causal thesis which is singled out as the primary target of those who are committed to the view (that thinking - problem-solving must, in the final analysis, be a linguistic exercise. Psychologist Zenon Pylyshyn, surely the most outspoken critic of the causal thesis, has this to say:

The extensive experimental investigations of imagery and imagery phenomena in the last decade, have been of unquestionable value in breaking through the earlier oppressive strictures on what phenomena ought to be studied. The empirical regularities demonstrated are of high reliability and wide interest, both from a scientific and practical point of view. None of these empirical results are questioned. The main question that is raised is whether the concept of image can be used as a primitive explanatory construct (i.e., one not requiring further reduction) in psychological theories of cognition.²

Similarly, but perhaps more definitively, Pylyshyn writes:

If someone asks me how many windows there are in my house and then asks me to report how I went about answering the question, this report (subject to the usual methodological precautions) may be taken as an accurate report of what I experienced doing. The only trouble is that I must give such a report in the only language I have available for describing my awarenesses. A

¹Alan Paivio, op. cit., p. 63. The parenthetical remarks are mine.

²Zenon Pylyshyn, "What the Mind's Eye Tells the Mind's Brain: A Critique of Mental Imagery," Psychological Bulletin, 1973, p. 2.

description in such a language may be entirely inadequate in meeting what Chomsky (1964) has referred to as "explanatory adequacy".¹

Pylyshyn's view of the causal thesis is echoed with equal clarity and conviction, by two other of Shepard's critics--John R. Anderson and Gordon Bower. Commenting on an experiment conducted by G.W. Baylor in 1971 designed to characterize how people solve puzzles by rotating visual imagery (an experiment which Anderson and Bower view as of a piece with Shepard's rotation experiments) they write:

The subjective counterpart of processing these symbolic descriptions of spatial information is that we are "seeing images" of successive parts of the puzzle, much as we would see an actual cube that we are dicing. There is no denying the validity of such subjective reports. What is to be cautioned against is the view that the subjective imagery explains the performance in any acceptable sense.²

Clearly, the findings of the rotation experiments demand explanation. The experiments point to the existence of a hitherto unknown fact about human behaviour. People who are making judgements about the shape similarity of two objects which have been rotated out of congruence with one another, exhibit reaction times which vary linearly with the degrees of rotation which would be required to bring the objects into superimposition. And they do this spontaneously and without fail.

¹Ibid., p. 3.

²J.R. Anderson and Gordon Bower, Human Associative Memory (Washington: V.H. Winston and Sons, 1973), p. 436.

The precision of this proportionality between reaction time and degrees of rotation is remarkable: according to Shepard's data, it takes a given subject exactly twice as long to react when the object pair display a rotational disparity of 100° as it does when the pair display a rotational disparity of 50° --just as it would if the objects were actually being physically rotated into positions of congruence. The regularity of the reaction-time pattern across subjects and its exactitude relative to degree of rotation, rule out the possibility that it just comes about--as a matter of coincidence. On the contrary, however odd, it is a fact--a fact about human behaviour which cannot be set aside or dismissed.

So the question is, do the findings of the rotation experiments entitle us to champion, in the manner of Shepard, Paivio, and Attneave, an analogue explanation for spatial problem solving of the requisite type? Or is there as Pylyshyn, Anderson and Bower, seem to suggest, something inherently false--odd perhaps--about this kind of explanation?

It must, I think, be granted in favour of the analogue explanation, that it has an immediate intuitive appeal. By this I mean that the correlation of the subjects' reaction times to the orientation disparity of the test objects can be readily seen to be a natural consequence of the operation of an analogue system of cognitive representation whereas in the case of a non-analogue system, a system in which the cognitive activity is essentially linguistic, such a correlation would seem to be decidedly unnatural or contrived.¹

¹In the mental rotation literature, the various commentators have utilized a number of different expressions to refer to whatever it is that an analogue or image system of cognitive representation is supposed

Consider briefly and in very general terms what it might mean to say (as Shepard does) that physical objects and physical processes are given cognitive representation in an analogue system. Two very distinctive characteristics of an analogue (as opposed to a non-analogue or linguistic) system should, I think, spring to mind at once. First of all, it seems likely that the basic structures or units of such a system would 'represent' by exhibiting some kind of structural similarity or isomorphism to the object(s) and/or situation(s) they are representing. There would be, that is to say, a likeness of pattern between 'represented' and 'representation'.

In this broad sense, an automobile fuel gauge can be considered to be an analogue system. The needle is related to the 'full mark' in the same way that the fuel in the gasoline tank is related to the top of the

to be running in opposition to. For instance, John Anderson and Zenon Pylyshyn describe themselves as defending the view that thinking-problem solving must occur within the confines of a propositional system of cognitive representation, via the formulation of propositional structures. Alan Paivio, on the other hand, argues that the kind of system of cognitive representation that the rotation experiments indicate does not operate during spatial problem solving of the requisite type, is a verbal system of cognitive representation. Again, other psychologists--Paul Kolers and William Smythe, for instance, describe the 'opposing' system of cognitive representation as a symbolic system of representation (Kolers and Smythe, "Images, Symbols, and Skills", Canadian Journal of Psychology, vol. 33, 1979). And Ned Block, editor of a recent collection of essays on the topic of imagery, describes the system as a descriptive system--the basic units of which are descriptions or, descriptive episodes (Ned Block (Ed.), Imagery, Cambridge, Mass.: MIT Press, 1981). All these expressions are, I take it, roughly equivalent to one another and correct as the authors in question use them. It is worthy of note that the generic distinction that they allude to is between an analogue or image system of cognitive representation (in which the units of representation mimic or model, in some sense, what they represent) and a non-analogue or linguistic system of cognitive representation (in which the units of representation do not).

tank. The relation in this particular case is quite literally a spatial one--as the quantity of the fuel in the tank gradually decreases, it becomes increasingly spatially distant from the top of the tank; likewise the needle on the gauge becomes spatially distant from the full mark. But the fuel gauge, it will be noted, is not a 'picture' of a gasoline tank. Analogue representations need not 'look like' in any straightforward sense what they represent--resemblance of the picturing kind is a nicety rather than a necessity in an analogue system.

Now in the particular case of imagistic-type representations in the brain, likeness of structure could well be taken to mean that each local portion (set of contiguous points) of the image would correspond to a portion of the represented object as seen from a particular point of view, and the interval relations among the portions of the image would implicitly represent the interval distances among corresponding portions of the represented object.

The second characteristic of an analogue system is that spatial problem solving is, strictly speaking, a non-inferential affair. In the broadest sense, this means that there occurs what might be described as a 'locating' of objects in space and this may involve the scanning across distances or the manipulation of structures or, very possibly, some combination of scanning plus manipulation. And the length of time it takes to arrive at a solution by way of these spatial techniques, is, in a very direct sense, a function of the distance involved in the scanning and/or manipulating. For example, a map of Canada is an analogue of the Canadian political and geographical reality. Any portion of the map is a representation of the corresponding part of Canada and distance on the map represents distance in reality. If we want to

know, by using the map, whether or not three cities--say Montreal, Regina, and Vancouver--fall in a straight line, (and, in so doing, determine the feasibility of visiting Regina on the way to Vancouver by car) we scan the map from Montreal in the east to Vancouver in the West or vice versa. And the solution time taken is a function of the distance covered in the scanning process. To put this as succinctly as possible, if Montreal and Vancouver were not so far distant on the map, if there were only three provinces instead of four separating them, then it would take a shorter time to complete the scanning process. Conversely, if Vancouver was even farther west than it is, the scanning process would require more time.

In the case of the problem situation stipulated by Shepard in his experiments, the subjects are required to determine whether or not the global shapes of two test stimuli are the same. Assuming that an analogue system of cognitive representation is being used in the solution process, and assuming that the general description of the nature of an analogue system given above is roughly correct, image-type representations in the brain would be rotated into a position of congruence with one another. And in this instance, solution time is a function of the number of degrees or 'distance' involved in the rotation process. The greater the distance of rotation, the longer the solution time must be. The point is, then, that if we assume Shepard's analogue explanation, the proportionality of the reaction times of the subjects participating in the experiments to the degree of orientation discrepancy of the stimuli, is not remarkable in the least. On the contrary, it is part and parcel of the logic of operation of an analogue system of representation, that the solution time (or reaction time) and the 'distance factor'

are in positive correlation.¹

But what of the alternative non-analogue explanation? What, in principle, would it mean to solve the shape assessment problem within the confines of the linguistic system of cognitive representation? Presumably, a linguistic system would not represent in the manner of pictures or fuel gauges or analogue computers. This is to say that the basic representative structures of this system would bear a relation to the things represented not by likeness (either in the sense of structural similarity or straightforward resemblance) but by convention. The sentence, 'The cat is on the mat', represents in this way. It conveys information about a particular state of affairs by its adherence to certain linguistic conventions--in this case, the conventions of English semantics, vocabulary and syntax and there is no 'likeness' to a physical cat positioned on a physical mat. The sentence is a description of that state of affairs.²

¹No doubt there will be some reluctance to accept the possibility of a 'rotation' taking place 'inside the head'. I think that sense can be made out of this idea in terms of computer operation, but for the sake of continuity, I would prefer to forestall discussion of this in detail until chapter 3. At the moment, I am simply working out the rough outlines of what analogue versus linguistic spatial problem solving amount to. I will say in passing, however, that this notion of the rotation of images in a brain space does not involve the conceptual difficulties inherent in the notion of rotation in a mind space. After all, the brain is a spatial entity--the mind is not. And there is no reason in principle why images or structures having spatial properties could not occur as topographic projections on the surface of the cortex.

²A word of clarification about this particular example of a linguistic representation. I do not wish to suggest by my use of it, that linguistic representations in the brain somehow necessarily assume the form of full-blown English sentences--that somehow exactly the same neurological states and processes are activated in thinking about a cat on a mat, as when the sentence 'the cat is on the mat' is said or written. On the contrary, it is more likely the case that the brain

Thus, a linguistic cognitive representation would not be like an image in the following respects: It is in no sense a topographic representation; parts of a linguistic representation need not correspond to parts of a represented object or scene, and distance between portions of a represented object or scene is not implicit in it. Rather, distance information (if represented at all) must be represented explicitly. Not only this, but shape, size, and orientation are independent parameters in the linguistic representation; it is possible to represent the shape of an object without also representing size and orientation. In the sentence cited above, for example, the word 'cat', gives us some fairly clear information as to the shape of the object in question (after all, cats have a characteristic shape) but we know little or nothing about the size and orientation of the particular cat being referred to. The cat could be large (like a tiger) or small (like a kitten) or it could be an 'archetypal' cat which is meant to represent cats in general and so be of no particular size. Similarly, the cat could be positioned on its back, or standing on all fours etcetera.

Furthermore, in a non-analogue system, spatial problem solving (of the specific type Shepard requires) would not be a matter of moving structures around in the brain or a series of brain states occurring at

generates a representational structure which is a short-hand of the sentence--generating perhaps structures for 'cat' and 'mat' only. Needless to say this is largely an educated guess about the specific form which linguistic representations take. Though there does seem to be an evolutionary justification for arguing this--short-hand representations would certainly have the value of efficiency which in turn has survival value. Also, observation of the early language behaviour of children would seem to indicate that the brain can and does represent in this short-hand way. Children regularly omit verbs and other parts of speech, communicating largely by the way of nouns.

a rate so as to create or provide for a continuous spatial pattern, or scanning across areas, but a matter of generating a descriptive account of each of the test stimuli in turn and comparing the descriptions for similarity. In other words, in such a system, there is no sense in which the solution is achieved in spatial terms. On the contrary, the problem solving would be a discursive process--stimuli are analyzed and described and inferences are made. And there would seem to be two possible inferences relevant to this particular circumstance: the inference that there is an exact part-for-part match between the two separately generated descriptions, therefore the stimuli must have the same global shape; or the inference that the two descriptions are not exactly the same, therefore, the stimuli are not the same shape. Clearly what counts in terms of the success or failure of non-analogue problem solving is the accuracy of the descriptions and the logical validity of the inferences.

Let us consider a particular case of spatial problem solving by way of a linguistic system of cognitive representation. Suppose, for the sake of simplicity, that one of the objects in the test pair presented by Shepard to his subjects (call it 'X') has a configuration much like that of the letter 'A'. Roughly speaking, this could be represented in a linguistic format by way of the proposition--'two lines meeting at the top to form a vertex and bridged about halfway down by a short horizontal line'. And suppose that the second object in the pair, (call it 'Y') looks something like an A on its side (\triangleright). The linguistic representation for this could be 'two lines meeting at their right-hand ends to form a vertex and bridged about half-way along the lines by a short vertical line'. Presumably, the comparison of these two descriptions

could be carried out on the basis of the following structural analysis: The descriptions of X and Y, respectively, make reference to the same number of lines--two lines to be precise, joined in each case at one end in a vertex. And each description makes reference to the presence of a short 'bridging' line approximately halfway along the vertex lines. The descriptions are precisely structurally similar, therefore the objects in question must be the same three-dimensional shape.

Now, in principle, there would appear to be no reason why the brain could not be constructed as to routinely make use of linguistic forms of representation and so function in respect of spatial problems in the manner described above. Not only this, but as the example shows there would appear to be no reason why, in the general course of things, a problem of shape similarity could not be resolved by the descriptive technique. Having said this, however, it must be acknowledged that strictly on a consideration of the 'logic of operation' of the linguistic system of representation, there would appear to be no reason for the proportionality of the subjects' reaction times to the orientation disparity exhibited by the test stimuli. If, as per our non-analogue hypothesis, what is happening is that the descriptions of each of the objects in a given pair are being generated and these descriptions are in turn compared for similarity, then the reaction times should be more or less a constant value. All things being equal, it should take no longer for subjects to respond in a 60° rotation case than in a 20° rotation case. The relative orientations of the objects in the pair is of no consequence to the descriptive process, for each of the object descriptions is generated separately. Thus it cannot seriously be argued that it should take longer to generate descriptions of the

objects in the case of a pair exhibiting considerable orientation disparity as opposed to the case of a pair exhibiting a minimal orientation disparity. The orientation factor is simply not taken into account, in the descriptions, in a way that has temporal ramifications. And, it is not taken into account in the comparison of the descriptions. It is the structural similarity of the generated descriptions which counts here.

Now that the analogue explanation turns out, as analysis, to be a better bet than the alternative, linguistic explanation, should not, I suggest, come as a great surprise to anyone. For the kind of mechanical problem solving strategy envisioned on this explanation is ideally suited to the resolution of the shape assessment problem. In fact, this is the kind of strategy that we would almost always choose to employ in such circumstances, should a choice be possible.

Let me be more specific. Suppose that we are participating in a series of Roger Shepard's reaction-time experiments and suppose that the exact details of these experiments are altered slightly. This time, we--the subjects--are required to perform our calculations of similarity/dissimilarity overtly, in the public forum, rather than silently or mentally. We are presented not with mere depictions of the test objects projected on a screen (as in the original experiments) but with actual three-dimensional models strategically placed on a table before us and affixed to the table by screws. We are told that the problem is to be solved either mechanically or verbally--in the first instance by moving the objects about until we are satisfied that a congruence of shape can or cannot be achieved, in the second instance, by giving a verbal description of each object in turn and then comparing the

descriptions (again out loud) for similarity. We are also told that speed is of the essence--the overall purpose of the experiments being the determination of the minimum amount of time required to make spatial judgments of this particular kind.

My question, then, is a simple one: can we predict, with any degree of confidence, which of the two strategies described above we will ultimately choose? Surely we can. Surely we do not have to wait upon the experimental results to see what will happen. We know already. We know that relative to the 'mechanical' rotation strategy, the verbal strategy is a patent waste of time and no one but a fool (or a show-off, perhaps) would even dream of attempting it. Quite simply, the verbal strategy is just too difficult, too complicated, and too liable to error to make it a serious contender here.

Prima facie, the analogue-rotation explanation for this kind of spatial problem solving makes sense. People solve the problem and exhibit reaction times which are precisely proportionate to the distance or degree of rotation required to bring the objects into congruence because they are, in fact, doing something which, in brain terms, is an analogue of the physical process of rotation. The linguistic explanation, on the other hand, makes no sense at all. If people are generating linguistic descriptions and making inferences on the basis of these descriptions, then there is no reason why their reaction times should be proportionate to the distance factor. To put it bluntly, the linguistic explanation, so-called, does not really provide an explanation for the phenomenon in question. Moreover, the analogue explanation is consonant with peoples' verbal reports that they did indeed rotate mental images. The linguistic explanation, it will be noted, is not.

2.4 The Metaphysical Thesis

Psychologists Roger Brown and Richard J. Herrnstein have sought to interpret the rotation experiments in terms of what they describe as the "common sense view". More specifically, according to Brown and Herrnstein, the findings of the rotation experiments warrant the conclusion that people have been right all along to insist on the existence of a mind's eye and to insist, further, that events occurring before this mind's eye--events such as mental rotation--are things/activities, as they put it, "in addition to" the physical events which are causally responsible for them.

Consider, once again, Brown and Herrnstein's statement of their interpretation of the rotation experiments:

The upshot was that there is an icon which is somewhat analogous to the physical stimulus itself. In an older vocabulary, based on personal experience rather than experimentation, we might have said that the data suggest a mental image of the physical stimulus. Common sense has always believed in a "mind's eye" in addition to the physical events in our sense organs. In that respect the research has just confirmed what people already know.¹

The thesis embedded here is metaphysical. Brown and Herrnstein apparently take themselves to be asserting a thesis about the status of the mental rotation phenomenon as compared to and, seemingly, as contrasted with the status of physical events. The telling phrase, in this regard, is the phrase "in addition to", as it clearly suggests a

¹Roger Brown and Richard Herrnstein, op. cit., p. 29.

separation of the categories of mental and physical. Equally telling is Brown and Herrnstein's assertion of a link to the common sense view, for that view, regarding mind in general and imagistic activity in particular, is quite unabashedly dualistic. Let me explain.

Suppose we set aside our philosophical training and preconceptions about mental phenomena for the moment, and consider imagery from the point of view of the casual, common sense image reporter. We shall be constructing, then, a speculative, common sense metaphysics of mental imagery. What interesting features, if any, can we report about mental imagery on the basis of our own experience of it?

One thing which cannot escape notice is that in imaging we seem to be confronted with something, to have something over against us or presented to us--and something other than the material object or physical event, real or fictitious, which we are trying to envisage. And it seems natural to say that we are aware of an entity, to use the entity-word 'image' to refer to it because we are experiencing something which behaves in an entity-like way.

The image shows a certain independence of us, a certain obstinacy or recalcitrance, almost as if it had a will of its own.¹ Often, it will not come when we summon it. It obtrudes itself on us when we do not want it, stays when we try to get rid of it. It may even intrude

¹It is interesting to note, in this connection, the report of Shepard's subjects (noted in the discussion of the original rotation experiment in Chapter 1) that there is an upper limit to the speed at which they could successfully undertake a mental rotation. (Shepard clocked this at 60° per second.) If they tried to rotate their images any faster than this, the subjects said, the images lost their structural integrity.

upon us and stay with us to the point of becoming an obsession. The image of a car crash we have witnessed continues to haunt our minds for hours, growing fainter at times, but still renewing itself, and it will not go despite all our efforts to get rid of it; and then at last it suddenly fades away of itself. What behaves in this way is naturally described as an entity, a particular. A noun substantive, such as 'image' is just the sort of word that seems to fit it.

Modern philosophers are continually telling us that mental images are not at all like pictures. But they are--at least when we consider them as they appear to us phenomenologically. It is true, of course, that a visual image cannot be handed around a table, as a photograph can. It cannot be torn up, or destroyed in a fire, though sometimes we may well wish that it could. It cannot be touched, nor seen simultaneously by different people from different points of view.

All the same, in some ways, it seems to be very like a picture. I am now imaging a mental rotation sequence, involving Shepard-type test objects. I am aware of something--an image of the test object on the left moving in a clockwise direction about the picture plane axis toward the object on the right. There has been a change of image orientation before my mind's eye. But I do not for a moment suppose that this orientation change is occurring or has occurred in the actual, physical test object pair. What changes is that presented and inspectable something by means of which the visualizing is done.

In a similar manner, it can be said that a picture has an independent career of its own. It changes when the object of which it is a picture does not change, or remains unchanged when the object does change. It continues to exist when the object has ceased to exist.

Conversely, it ceases to exist, while the object endures. Thus in having a history independent of the history of its original, a mental image is like a picture. It is also, of course, like a picture in having a more or less close resemblance to the object or event of which it is an image; and it is like a picture, again, in being a means by which we can think of its original.

But where are my images? What space do they inhabit? The answer to this question is a paradox--they seem to be somewhere and yet at the same time nowhere. Clearly, my visual image of the completion of a given mental rotation is not anywhere in the physical world. Yet it appears to me to be spatially extended. Its parts are spatially related to each other by relations of location, and also by relations of larger and smaller, and orientation. And the rotation process itself, as it unfolded before my mind's eye, appeared to be a spatial process. But, I have to say that the imagery is not in my head either in the sense that I might say, it is two inches from my left ear or behind my nose. It occurs, if anywhere, in the unusual space (for which no dimensions seem appropriate) that we call the mind--wherever that is.

Some images, though not my mental rotation image, are 'projected', and do seem to have a place, not, indeed, in physical space, but in the space of the percipient's visual field. For example, a visual image of a long lost relative can be projected in front of the door. Here it seems reasonable to say that the image is 'somewhere', at least, somewhere in the percipient's visual field. Even so, it is 'there'--in a very peculiar way. For, is it not, visually impenetrable. Although the 'relative' image is at the door, no part of the door's

surface is rendered invisible, as it would be if the real person actually stood there.

Another interesting feature of mental images is that they seem to make possible--causally possible--certain kinds of behaviour, most particularly, problem solving or question answering behaviour. On being asked whether a pair of Shepard's test objects are the same shape I can refrain from performing the calculation by actually physically rotating one object into congruence with the other and, instead, with the help of mental imagery, perform this activity mentally. As to what physical events in the brain go on as a correlative of this mental activity, all I can say is that I know nothing of them. At the time of the rotation, I simply do not experience them. They are, what, I suppose, if pressed, I would be prepared to infer are the remote causal ancestors of the experienced mental rotation.

There are several things worthy of note in this speculative account relative to our original query regarding the dualist tendencies of common sense. First and foremost among these, is the descriptive language which has been utilized. I have spoken of 'mental pictures', 'mind's eyes', and 'mental images'. These are, as I have already noted, entity words and they clearly suggest that image phenomena have, in some vague sense, an entity status. But what, precisely, is this status? Apparently, it is unusual and even sometimes paradoxical. Images are entities but they are entities unlike the physical objects they supposedly represent--they are private, immaterial and, even more surprisingly, they are spatial, but not in physical space. Con-juring up a large elephant before one's mind's eye does not mean that anything weighing several tons is literally--physically within the

confines of one's head. Nonetheless, according to the view espoused above, there is still something there--in the mind--something big and fat. And what of the process of mental rotation? Mental rotation, it seems, is an experienced event--something we see unfolding before the mind's eye. What goes on in the brain, on the other hand, as a correlative of this, is quite a different matter. It is a distinct event--presumed to be the material cause of the experienced mental rotation. So, there are really two events to be counted here--the experience of mental rotation, and whatever it is that occurs in the brain to cause this experience. (See Appendix A.)

Clearly, this 'two kinds of entity'--'two kinds of process' view is dualistic. Equally clearly, by linking Shepard's research findings to such a view, Brown and Herrnstein have, in effect, asserted a confirmatory connection between these findings and metaphysical dualism. What Brown and Herrnstein appear to be supporting, in the final analysis, is the view that the results of the rotation experiments prove that mental rotation per se has a special metaphysical stature--that it is a mental, as opposed to, physical, process. The question is, then, do the rotation experiments warrant this interpretation? Do they have the metaphysical implications that Brown and Herrnstein apparently believe that they do?

The short answer to this question is, no. The rotation experiments do not vindicate the metaphysical dualism of the person of common sense. That people are able to make judgments of shape similarity without utilizing any external visual or tactual aid, and that the time it takes them to do this is precisely in proportion to the number of degrees that one object in a given pair must be rotated in order that

it might be brought into congruence with the other, and that, as a correlative of all this, people consistently report an experience of mental rotation, certainly tells us something about the nature of the problem solving activity--that it must, in some sense, be an analogue of the physical process of rotation--but it does not tell us that that activity is mental and not physical. Equally compatible with such data is the view that mind and brain are identical and that what we have in the case of the mental rotation phenomenon, is simply an experienced neurological event of a particular kind.

What Brown and Herrnstein have seized upon here, clearly, is the fact that the linear reaction-time pattern (which is significant of analogue character of the problem solving activity) is consistently coupled with the verbal report of experienced mental rotation. They have inferred that this entails something about the metaphysical status of the problem solving activity--that it is essentially an experienced event. The consistent correlation of verbal report and reaction-time pattern is, of course, striking and does invite speculation and, it must be granted, that it is just what we would expect to be the case if dualism per se were indeed true. If spatial problem solving of the appropriate type is to be categorized as an experienced mental activity, then people should be aware of mental rotation--they should be able to monitor it introspectively--whenever it occurs. But we should also expect this correlation of reaction-time pattern and verbal report if people were simply experiencing certain alterations in their neurological states. We should expect, in fact, that people experience these processes in a way that roughly captures the nature of the processes.

In relation to the metaphysical issue, then, it seems to me that the results of the rotation experiments are neutral. It may be that a dualistic interpretation of the rotation phenomenon is appropriate and it may not be. The results of the experiments do not seem to constrain us either way.

However, that the rotation experiments fail to vindicate the metaphysical thesis, in no way signifies that Shepard's overall programme of research is, in some crucial respect, flawed. It is one thing to adduce evidence regarding the properties of the particular system of cognitive representation operative in a problem solving sequence (which, I take it, Shepard does), but quite another to settle the mind-brain issue in favour of dualism, which is what would be required if Brown and Herrnstein's interpretation of Shepard was true. Indeed, it would be unrealistic to expect that scientific data alone could accomplish this latter task.

People hold positions in metaphysics for various a priori reasons--most basically, because one position or the other happens to be a better fit with the overall conceptual scheme which they have come to adopt. I expect that philosophers in particular, find the conceptual divide between 'the mental' and 'the physical' to be very impressive. The language of 'mental' and 'physical' seems to describe two radically different kinds of entities and two radically different kinds of events. And so they would be quite naturally reluctant to accept the likelihood of any kind of causal interchange between a 'physical' rotation process and a 'mental' rotation process. Unless such a priori convictions can be addressed directly, it is almost a surety that the data of Shepard's rotation experiments will be read (by philosophers and by anyone else

so inclined, metaphysically speaking) unsympathetically in terms of the dualist thesis and sympathetically in terms of the identity theory. And, as I have already argued, there is no reason, at least none so far as the rotation experiments are concerned, why these a priori convictions should not carry the day.

2.5 Conclusion

In sum, then, I have argued that, of the three theses of interpretation put forward by Shepard's colleagues in cognitive psychology, only one--the metaphysical thesis--ought to be rejected. Brown and Herrnstein have come to adopt this interpretation apparently, more on the basis of their own a priori convictions about the nature of mind than on the basis of any clear evidence for dualism (or for identity, for that matter) which the rotation experiments bring to bear. The other two theses of interpretation--the quantification thesis and the causal thesis--I have argued, are, indeed, warranted by the results of the rotation experiments.

CHAPTER 3

Image Rotation as Experienced Brain Process

3.1 Introduction

In large measure, Shepard is himself responsible for the dualist interpretation of the rotation experiments given by Brown and Herrnstein. The reason for this, I would argue, is that Shepard is simply not interested in addressing conceptual issues in any systematic fashion and so he has failed to state in clear terms just what he takes image representations and the rotation phenomenon to be. What he does in fact is confuse the issue altogether by describing such phenomena in two distinct languages--in the mentalistic language of common sense, and in the physicalistic language of neurophysiology. And as readers, we are left to draw our own conclusion as to what Shepard takes to be the salient description.¹

¹Shepard's omitting to declare himself unequivocally in this matter is not merely an artifact of his personal mode of presentation. Cognitive psychologists, generally, seem not to worry about the mind-brain problem. It is not entirely clear why this is, but philosopher Owen Flanagan has proposed a plausible explanation. "In some quarters," writes Flanagan, "there is the view that the mind-brain problem can simply be ignored. New law-like generalizations about cognition are being discovered every day by scientists who worry not one iota about the metaphysical nature of the underlying process." (Owen Flanagan, The Science of the Mind (Cambridge, Mass.: MIT Press, 1985, p. 214). Apparently the attitude of cognitive psychologists can be summed up in the idea that, in science, "nothing succeeds like success."

For example, in a discussion of an experiment, conducted by M.I. Posner, designed to determine subjects' reaction times in a pattern recognition task, Shepard has this to say about image representations in general:

... these experiments appear to furnish rather strong evidence concerning the nature of particular internal representations - specifically whether they are principally visual or verbal in form. However, the question still remains whether these particular internal representations or "codes" are what we ordinarily refer to as images.¹

Now I take it that the implied contrast here is between image representations conceived as brain states and image representations conceived as a kind of experience. By way of the terms "internal representations" and "codes," Shepard appears to be suggesting the former--a state or a structure which is neurophysiological in nature, and by using the descriptive phrase "What we ordinarily refer to as images," Shepard would seem to be alluding to the common sense conception of imagery--that it is a particular kind of experienced episode. This appears to be the force and significance of the word 'ordinary' in this context.

But this is the only occasion on which I find Shepard actually putting the two kinds of description into juxtaposition--and offering, thereby, a kind of comparison and contrast. As a rule, Shepard simply shifts back and forth without warning or justification between the two. Sometimes Shepard describes image representation in the language

¹R. Shepard and Lynn Cooper, 1982, op. cit., p. 73. (The underlining is mine.)

of physics--he is inclined to describe it, for instance, as a 'system' or 'faculty',¹ achieved in the human species through a process of evolution and which can simulate spatial transformations in the physical world. Or, when he is drawing conclusions from one or the other of his reaction-time experiments, he describes the nature of the image representation in the following terms:

Although we can not yet say anything specific about the representation of three-dimensional objects at the neurophysiological level, our results do seem to place some significant formal or abstract constraints on the nature of that representation. Perhaps the most important of these is that the internal representation embodies important structural features of the three-dimensional object that are not manifest in the two-dimensional projection of the object on the surface of the retina.²

And,

Such an internal representation cannot adequately be regarded as an undifferentiated neural event (such as the activation of a particular neuron or population of mutually interchangeable neurons) ... Instead such a representation must have an internal structure that is itself to some extent analogically related to the structure of its corresponding external object. For during the process of rotation, the parts and relationships among the parts must be transformed in very constrained ways in order to enable the kind of rapid, template-like match against an ensuing visual stimulus that we have demonstrated here.³

¹Ibid., p. 1 and 3.

²Ibid., p. 69.

³Ibid., p. 120.

At other times, however, Shepard gives image representations an experiential content. In several of his articles and in his book, he spends some time recounting 'famous' introspections of imagery such as Albert Einstein's epochal Gedanken experiment of imagining himself travelling along with a wave of light at 186,000 miles per second¹ or the German chemist Kekulé's report that he experienced spontaneous imagery during idle reveries, of atoms joining and rejoining to form chainlike molecules, "whirling in a giddy dance."² And Shepard speaks of his own experience during a period of hypnopompic suspension between sleep and waking of a "spontaneous kinetic image of three-dimensional structures magestically turning in space."³ At such times, he is given to a recounting of the phenomenological flavour or feel of the imagery experience. And he reverts quite obviously to the language and expression which the common sense introspector naturally invokes when he is giving his reports. Shepard makes use, for example, of expressions such as the 'mind's eye' and he speaks of 'seeing' pictures before the mind's eye. Indeed, it is in just such introspective terms that Shepard describes Michael Faraday's original 'image-insight' into the nature of electric and magnetic fields. He writes:

What kind of "intuition" or "instinct" was this? A clue may be discerned in this; the invisible lines of force, which Faraday visualized as narrow tubes curving throughout space, rose up before him like things.⁴

¹Ibid., p. 6.

²Ibid., p. 7.

³Ibid., p. 7.

⁴R. Shepard, 1978, op. cit., p. 126.

Now this is virtually guaranteed to warm the heart of any would be dualist. That Shepard has seen fit to employ two distinct languages of description will be taken to be significant of his intention to mark a distinction between two kinds of representational phenomenon. It will be assumed, that is, that, for Shepard, there are experienced images and experienced rotations 'in addition to' physical images (in the brain) and physical rotations. Equally telling perhaps from the dualist perspective, will be the fact that experimental research on imagery has a particular history. Hitherto, those who engaged in this research--psychologists like E.B. Titchener, for instance--fell decidedly into the dualist camp. Thus, since there has always been a connection between a study of mental imagery and dualism and since Shepard says nothing clearly to the contrary, it is an easy (though, false, as I have argued earlier) inference to make that Shepard is himself following the tradition established by his predecessors, and that his research has something to do with proving the truth of metaphysical dualism--at least in respect of the one kind of experienced phenomenon.

I would like to suggest that Shepard's comments be set aside. I suggest this for two reasons: first, because it is obvious that Shepard has nothing particularly illuminating (from a metaphysical point of view) to tell us about the rotation phenomenon--(apparently, he has simply not worked this out for himself with any degree of clarity or detail); and, second, because it really doesn't matter what Shepard himself believes or how clearly he expresses his belief. The important thing, in this circumstance, is to establish the kind of account of rotation which will best enable Shepard to sustain his overall analysis. The rotation experiments demonstrate a causal connection between the

phenomenon known as mental rotation and the problem solving activity of Shepard's subjects. But how can this be? The rotation experiments themselves do not explain just how this causality is effected--or just what its nature is.

In this chapter, I shall attempt to work out a theoretical model for this--a model of the kind of causality which makes sense out of the asserted connection between a particular kind of experienced activity and spatial problem solving. In particular, I shall be arguing that the experience of image rotation is best not construed on the dualist analysis. Rather, that experience is identical with physical processes in the brain. Later in the chapter, I shall discuss the consequences of adopting the 'image rotation-as-experienced-brain process' model for Shepard's experimental method and I shall be particularly interested in discussing this model in relation to introspective reports of imagery.

3.2 A Model for Image Rotation

On the strength of the reaction-time evidence adduced in the rotation experiments, Shepard has argued that the rotation phenomenon is causally significant. According to Shepard, people solve the shape assessment problem by generating image representations of the test objects and by rotating them into positions of mutual congruence (or non-congruence). There are several possible ways in which this assertion of 'causality by image rotation' can be construed.

In the first instance, we might adopt a literal interpretation of the assertion. We might take it, that is, that it is peoples' experience of image rotation which counts causally in the problem solving

process. If people did not see before this mind's eyes, images in rotation, if they were not able to monitor the progress of such rotations, they would be unable to solve the problem at hand and, at the same time, exhibit the appropriate reaction-time behaviour pattern. On such a view, the experience of the rotation is the necessary and sufficient condition for the problem solving behaviour.

Now given that Shepard has established that the experience of a rotation consistently accompanies the problem solving behaviour, a case might well be made out--not a case of strict logical entailment, of course, but a good probabilistic case--for the view that the experience of rotation is a necessary condition for the spatial problem solving. Having said this, however, it is not at all clear that we would be entitled to say anything about the sufficiency of that experience relative to the problem solving. In point of fact, the assertion of sufficiency here would require a preparedness to accept that causality between the spheres of things/activities mental and things/activities physical is possible and, indeed, does, in fact, occur. It requires, that is, a preparedness to accept some version of the Cartesian interactionist picture--specifically that imaging a rotation is sufficient to produce certain electrical and chemical changes in the brain, which, presumably, produces in turn, the subjects' pressing of correct response levers at a specified time interval.

There are real problems with this picture of causality. After all, if as the person of plain common sense and the Cartesian insist, 'the mental' and 'the physical' per se are essentially different, it does not seem reasonable to expect that one could give rise to the other. The human brain is a physical organ, it occupies three-dimensional space and

operates on the basis of electrical and chemical fluctuations/alterations in its physical substance. Surely it makes no sense to suppose that something which is neither electrical nor chemical, indeed, something which has no physical substance whatsoever--a 'mental' rotation--could produce these changes.

As an alternative to the literal interpretation of causation by image rotation, we might adopt an 'epiphenomenal' interpretation. We might say, that is, that the experience of image rotation is a mere by-product--an epiphenomenon--of certain events in the brain which are the real causes of the spatial problem solving. On such a view, the experience of image-objects rotating before the mind's eye might be likened to smoke which escapes from a factory chimney during a manufacturing process. The smoke itself has nothing causally to do with the actual creation of the product, it is simply a by-product of the operation of machines and the burning of fuel which are causally significant.

It could be, of course, that what people apparently experience during spatial problem solving sequences are just effects of brain states and never causes of anything. This is a logical possibility which is not entirely removed by the fact that the experience and the problem solving behaviour always occur together. But, then, it seems reasonable to ask why this should be so. Why should human beings have evolved in such a way that they consistently have mind's eye experiences of objects in rotation when they are engaged in problem solving of the appropriate sort when, so the epiphenomenalist would have it, such experiences have nothing whatever to do with the problem solving, and in fact are mere distractions from the real causal events? Indeed, one

is inclined to wonder why we should have evolved with minds and mind's eyes at all, if such things are utterly inconsequential.

Even the factory smoke, which plays no role in the production process, does have some effects--effects which are, in some instances, very important to trace. Smoke can be seen and smelled, for instance. Also, it can cause your eyes to sting. It can harm plant and animal life, interact with moisture in the air to form smog, darken windows, and so on. But experienced rotations, according to the epiphenomenalist, can do nothing of the sort. They quite literally do nothing at all. If that is true, then they are like nothing else in the universe. They are absolutely useless. But if everything else in the universe plays some role in its working why should mental states--the state of mental rotation--be different? Surely, in evolutionary terms, epiphenomenalism, in regard to the image rotation experience, is a gratuitous assumption.

Additionally, it seems to me that epiphenomenalism is no more coherent a thesis of causality than is Cartesian interactionism. What it is, in fact, is simply one side of the Cartesian coin. On the epiphenomenalist interpretation, brain events supposedly cause (occasion) mind events (epiphenomena), but these latter are themselves entirely inert. But if, as I have argued above, there are real problems about how mind and brain, 'the mental' and 'the physical' interact generally, such problems are not removed by restricting the causality to one direction only--from brain to mind. An explanation is still required as to how this causality could be effected given that mind and brain are different kinds of things obeying different laws and principles of operation.

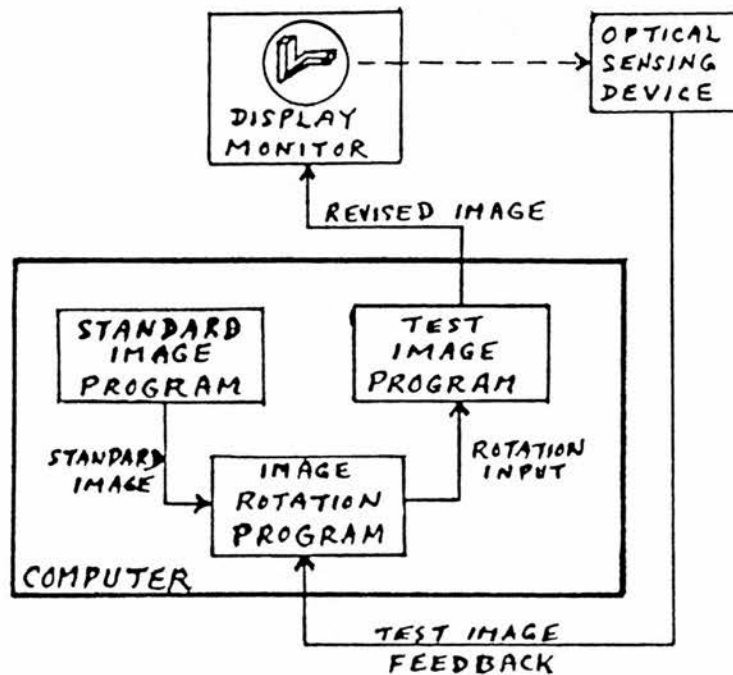
And, finally, it will be noted that the epiphenomenal interpretation of causality by image rotation is contrary to the spirit of the overall analysis which Shepard has been concerned to offer. Whatever ambiguities of language we may find in this analysis, Shepard appears to be adamant on one point--the rotation phenomenon--the phenomenon that people report--has a causal role to play in the spatial problem solving. Says Shepard, people generate these image objects, they cause them to rotate, and they monitor these rotations before their mind's eyes.

There is a third possible interpretation of causality by image rotation. Let us suppose that the identity theory of mind is true. On this supposition, image rotation comes under an entirely new description. It is not a mental--as opposed to, or in addition to--physical event, but it is an experienced physical event--call it, perhaps, 'X' series of physical events occurring in/on the human brain. In principle, the causal connection between this physical series and the ultimate physical series for which we are seeking causal explanation--the pressing of the correct response lever at the specified time interval--is not a problem. There are no ontological chasms to be bridged. Electrical-chemical events in the brain--the image rotation--bring about other electrical-chemical events which in turn, act through the central nervous system to produce the requisite muscular movement and to activate the speech centre.

Now suppose we take this identity interpretation one step further. Suppose, that is, that we conceive the operation of the human brain along the lines of modern digital computer with feedback capability. If we do this, the causal significance of the individual's experiencing--monitoring his/her image rotations becomes obvious. Consider the

following schematic model of computer operation.

Figure 6



Computer Model of Image Rotation Process.

The basic components of this model are three: the computer itself (with three programs), the display monitor, and the optical sensing device. The computer is programmed with information about the standard figure. This information is stored in the computer memory. At the

beginning of a given problem solving sequence, information about the test figure is programmed into the computer and both figures are projected in the form of images onto the display screen--the standard image on the left and the test image on the right. The sensing device scans ("sees") the visual information, identifies each of the patterns, notes the relative orientation discrepancy and sends this information back to the computer program for image modification. A modification signal is emitted and the test image is rotated on the display screen a specified number of degrees. The sensing device continues to monitor this process, noting the gradually decreasing orientation discrepancy, and sends the information back to the computer which in turn continues emitting the appropriate modification signals. This process of rotation and monitoring continues until the sensing device detects no difference in orientation between the standard and test image. At this point, the feedback output is 0.

In this general scheme of things, the role of the generated image and the sensing device is crucial. The computer needs the visual information in order to determine when and how much modification of the generated image is required. And depending on just how the test image looks relative to the standard, the sensing device either will or will not feed information back to the computer.

In human terms, the computer is the analogue of the brain and the image display and the sensing device are the mental image and the mind's eye, respectively. Like the image on the display screen, the mental image undergoes a process of gradual transformation--it changes spatial orientation. And like the sensing device, the mind's eye monitors, and, ultimately, guides the speed, direction and degree of that

transformation.

Does this mean that there are, in some quite literal sense, objects moving about in/on the brain? I do not think so--at least no more than there are really objects moving around on the computer display screen. Certainly it 'looks like' there are objects, but the truth of the matter is that the computer hardware is digital. There are no objects per se, but a complex series of electrical events--each event constituting a unit in its own right. Together, these events are 'seen' by the sensing device (and, indeed, by the naked eye) as a single entity. And it is this 'seeing' of the electrical units, as comprising a whole, a single image-object, which counts causally for the spatial problem solving. That one perceived whole--the test image on the right--eventually comes to achieve a match with the other perceived whole--the standard image on the left--produces, in turn, (again by digital electrical activity) the final problem solving behaviour.

In a similar manner, it seems reasonable to suppose that experienced images in the brain are like the images on the display screen, digitalized at a first stage. And then, upon that field, epistemic sortings out may or may not be performed. These, obviously, are at another logical level completely. One can fitly compare here the digitalizing that produces a pixel-picture (made up of tiny black-and-white squares) in a newspaper, with what one chooses or does not choose to see in that picture as a logically distinct entity. In terms of neurophysiological hardware, images are not static structures which can be spatially located and excised out as a single unit; they are a fairly complex series of electrochemical events. These 'events' are seen or experienced by the mind's eye as a single image. That they are

experienced in this fashion produces the unique reaction-time behaviour. People see images--whole images--gradually rotating into positions of mutual congruence and so come to solve the shape assessment problems in the length of time it takes to complete this wholistic process.

What about the mind's eye? Is there a separate mechanism, a part of the human brain corresponding to this? There may be or there may not be. Mind's eye scanning might well be best described as a stage in the overall processing rather than as a distinct structure as it clearly is in the computer model. Perhaps the best way to account for the mind's eye, in terms of brain hardware, is to say that in imaging a rotation, what is going on neurophysiologically, whatever states and/or structures are activated are the same states and/or structures which are activated when the experienced rotation is taking place externally, in physical terms, only, in the latter case, the activity is activated from the outside. (See Appendix B.)

3.3 Methodological Implications

I have argued that image rotation ought to be conceived on the identity model. This model has been generated out of the need to provide for Shepard a suitable metaphysics of image rotation--a way of accounting for its apparent causality in spatial problem solving sequences. It seems to me that this model bears further consideration in regard to its methodological implications. In particular, I want to consider the implications for introspection of taking image rotations to be experienced brain processes.

Suppose that an individual, hand in pocket, claims that he is holding several coins. We cannot tell for certain whether or not this is true as we cannot see the inside of the pocket. Yet, all things being equal, we will be quite prepared to grant a certain epistemic authority to the individual's claim. We will reason that he is in a good position to know the facts of the case. After all, people, hands in pockets, ought to be able to feel and to identify familiar objects such as coins. And unless we have some reason to believe that the individual is lying to us, or that he has recently suffered a minor stroke such that his hand is now paralyzed and incapable of grasping and feeling small objects such as the coins, it will seem reasonable to us to treat the report in the spirit of a true observation, and guide our activity accordingly. We might, for instance, proceed to the coffee shop with the individual fully expecting to have refreshment purchased for us.

Now is the case of coin reporting really substantially different from the case of the individual who reports that he has just solved a spatial problem by mental rotation? Is the latter individual not equally in a good position to report on his problem solving activity? On the identity model of image rotation, he certainly seems to be. Given that the experienced rotations are conceived to be identical with the brain states activated in the problem solving sequence, the report of having such experience must be recognized as a kind of observation of the actual problem solving, or, at least some stage of it, in principle not different from the individual's observation of the coins in his pocket and, therefore, as a legitimate source of knowledge in the matter.

It will be noted that the epistemic authority being granted to the introspective report of problem solving by image rotation is not the kind traditionally ascribed on the basis of a Cartesian analysis. To be sure, the individual making the report has a 'privileged access' to the problem solving activity--no one else can experience it. But this is no more privileged than is the access which the individual has to the coins in his pocket. It is not the case that the rotation activity--as 'mental' activity--is intrinsically better known to its possessor than anything physical could possibly be known to anyone. Reports of image rotation problem solving are not incorrigible. Individuals could--like the hypothetical stroke victim, who is unable to feel and identify the coins in his pocket--be suffering from some kind of neurological deficiency or damage which makes physically impossible the experience of the appropriate kind of brain state. In this event, the introspective report of image rotation problem solving could not be considered to be genuinely observational. Barring such possibilities, however, the introspective report is observational. And the individual making it, is necessarily in a good position to provide information about the nature of the problem solving activity undertaken.

It is clear, then, that on the identity model, introspective reports of image rotation are not just so much useless bag and baggage--mere sounds, as the behaviourist would have it, the cause of which may or may not be investigated. On the contrary, given the view that image rotations are experienced brain states, the cognitive investigator has every reason to seek out reports of such experience and to take these reports quite literally and seriously as indicative of the nature of the problem solving process. And they should constitute very useful

starting-points and guides for further experimental research.

Notwithstanding the fact that introspections of image rotation and of image phenomena in general could be conceived on the identity model, and treated in this 'observational' fashion, most contemporary cognitive psychologists, (presumably harbouring behaviourist scruples) remain suspicious of introspective reports of imagery and sceptical of their value as empirical data. For example, cognitive psychologist Gordon Bower has claimed that "people utter an awful lot of nonsense about their mental imagery.... The normal person's introspections are frequently neither very discriminating nor particularly valid...."¹ Ulric Neisser has suggested that asking subjects for introspections of their mental imagery is "a little like asking them to describe shifting cloud formations of a stormy sky."² And D.F. Marks has argued that mental imagery ought not to be defined by reference to conscious introspection. Says Marks, "The mental image has been allowed to reenter the arena of scientific psychology only on the condition that it be stripped entirely of its mental and phenomenological connections."³ And, even Alan Paivio, himself an image researcher and a strong supporter of Roger Shepard's work, denounces introspection as a possible technique for data gathering in cognitive science. He sees it, rather, as a technique for

¹Gordon H. Bower, "Analysis of a Mnemonic Device," American Scientist, vol. 58, 1970, p. 498.

²Ulric Neisser, "Changing Conceptions of Imagery," in P.W. Sheehan (Ed.), The Function and Nature of Imagery (New York: Academic Press), p. 238.

³David F. Marks, "Individual Differences in Vividness of Visual Imagery and Their Effect on Function," in P.W. Sheehan, *ibid.*, p. 92.

hypothesis gathering. Paivio writes:

Consider, first, the role of conscious experience.... This is no longer used as a basis for defining the functional attributes of imagery and for this shift we must be grateful to John Watson. If used at all, introspective evidence primarily supplements other information in supporting the inference that imagery is involved in task performance. Subjective reports might also be used as an informal source of research ideas, but they are not used to define the fundamental properties of imagery....¹

Significant here is Paivio's assertion of a distinction between the utilization of introspections of imagery as heuristic devices (or, as Paivio himself puts it, as an "informal source of research ideas") and the taking of these introspections to reveal functional qualities of image-type representations and image processes. For Paivio, apparently, introspections of imagery can be used in the former sense, but not in the latter. On such a view, mental imagery and image processes such as rotation, are really nothing more than theoretical constructs--an entity construct and a process construct, respectively--which the investigator postulates in order to get the experimental process in motion. And the constructs themselves are entirely expendable--the investigator is quite prepared and in some instances even expects that the 'objective' evidence so-called may not support their postulation. Even so, Paivio contends, the investigator may do well to take them seriously in an experimental framework and see what may come of

¹Alan Paivio, 1974, op. cit., p. 50.

them.

For example, it may well be scientifically useful, Paivio would argue, to treat as true peoples' introspective reports that they generate pictorial representations before their mind's eyes and that they rotate them in order to conduct shape assessments, even though the investigator himself may believe, for other reasons, that this is unlikely to be quite accurate as a statement of causal activity. Such introspective reports can constitute a starting-point for scientific research, a basis out of which experimental hypotheses (hypotheses such as the 'distance of rotation--length of reaction time hypothesis' adduced by Shepard and Metzler in 1971) can be generated and out of which, through the testing process, discoveries can be made.

The ultimate justification for such an experimental procedure, therefore, is really a posteriori--by way of the test results achieved. If the results sustain the hypothesis being tested, then the investigator has good reason to infer certain functional properties. In the case of the rotation hypothesis, positive results enable the investigator to infer that there is indeed something like a rotation going on. Alternatively, negative results, might well enable the investigator to make a different kind of inference. If the subjects' reaction times varied not with the distance of rotation, but with the structural complexity of the objects in the pairs, for instance, then the investigator might infer a linguistic problem solving strategy. Either way, something has been discovered which constitutes a useful basis for inferring some functioning phenomenon--the image rotation or, (with the negative results) the linguistic analysis--but this phenomenon, it will be noted is, at no stage in the process, observed. Paivio writes:

All of the more useful evidence on the nature of imagery comes from objective methods in which the involvement of imagery in a given task is encouraged by the use of relevant stimulus materials such as pictures or concrete words, by giving subjects instructions to image and so on. These procedures alone do not reveal the functions of imagery nor are they intended to do so. The inferences about function are based instead on the measurable relationship between the demands of the task and how subjects perform on it.¹

The identity view that I have been advocating runs contrary to this. Introspective reports of images and of image rotations are not just heuristics, utilized in an experimental framework, as Paivio and others would have it, in the hope that, fortuitously, some empirical fact of interest will be uncovered experimentally. The use of such introspective reports is to be justified not just by their results (which in the case of Roger Shepard's research happen as a matter of fact to be significant and happen to warrant an inference to rotation activity) but, also, and indeed primarily by their particular nature--by virtue of the fact that the experienced rotation is a brain process in operation.

Thus, it could be said that, on the identity view, image rotation per se is already well-grounded on an observation basis, and that what the rotation experiments accomplish is the provision of a second, inferential basis for ~~postulating~~ image rotation. This second basis helps to confirm what people already know introspectively. Additionally, the experiments provide further data which help to explain and elaborate the nature of the rotation phenomenon and a basis on which the

¹Ibid.

investigator might make judgments about how felicitous ordinary language introspective references to 'mind's eyes', 'pictures' and 'images' and about descriptions of images moving about in a rotation fashion really is.

There is one final point I want to make in this context. It seems to me that Alan Paivio, Roger Shepard and anyone else seriously interested in conducting cognitive investigations would be well-advised to take the metaphysical plunge that I have been advocating here. It is all well and good to say, as Paivio does, that psychologists can proceed experimentally on the basis of introspective reports even though they do not, for the most part, believe that these reports have any special observational credentials. In the practical circumstance, they will not do so. Unless the investigator believes that there is something in an introspective report, something in the fact that people consistently say that they see or do such-and-such in these circumstances--unless the investigator believes that this report is revelatory in some degree of a fact of cognition--it makes no sense for him to pursue it as if it were true. And Paivio is just kidding himself if he believes otherwise.

3.4 The Problem of Demand Characteristics and Experimental Design

Psychologist Martin Orne¹ has alerted cognitive investigators to the danger that many of their experimental results may be attributable to the 'demand characteristics' of the experimental setting. That is,

¹Martin Orne, "On the Social Psychology of the Psychological Experiment: with Particular Reference to Demand Characteristics and their Implications," American Psychologist, vol. 17, 1962, pp. 776-783.

the subjects may deduce the purpose of the experiment in which they are participating and may manipulate their responses so as to give the experimenter the results they think he wants. This is easily done, says Orne, if there are no objective performances required and so the response in question is exclusively a verbal one. In the case of imagery research, Orne notes, subjects are typically instructed to provide a response only after a certain condition has been met in an image--a condition that is detectable to the subject and the subject alone. The subjects may be told to respond, for instance, when they have seen a particular object in an image, or when they have transformed the image in some way. Orne argues that, in such circumstances, it is quite likely that the resultant verbal report is more a function of these demand characteristics than of any genuine observation that the subjects may have made regarding their problem solving activity.

Obviously, Orne's argument cuts against the position I have adduced above in connection with the observational status of introspective reports of image rotation. For, if Orne is right, then it really doesn't matter whether or not the introspective reports of image rotation are the products of conscious monitoring of events in the brain and so, in principle, not essentially different from any other observations, because in the practical circumstance, whatever observational value they may have is bound to be eroded by the influence of demand characteristics--by the fact that people will simply tell the investigator what they believe they ought to tell him. Using introspective reports as if they could constitute data, as if they could be evidence for anything at all is, therefore, a suspect scientific practice.

The demand characteristic problem was originally identified in the early part of this century through the conflict now known as the 'imageless thought' controversy. America's most famous introspective psychologist, E.B. Titchener, believed that images played a necessary and essential role in thought; indeed, for Titchener, thought simply was a train of images (not just visual ones of course, but auditory, kinesthetic, and other images as well). Workers in his laboratory were trained to be sensitive to the nuances of their internal lives, and spent many hours performing relatively simple tasks, repeating actions and experiences over and over until they were able to reduce their thoughts to their fundamental imagistic components. Unfortunately, some of Titchener's contemporaries came to the opposite conclusion. In several influential experiments, the psychologists of the 'Wurzburg School', under the directorship of Oswald Kulpe, asked their subjects to introspect on the contents of their minds while in the act of trying to answer difficult questions. These subjects (mostly psychologists themselves) were often as vigorous in denying the presence of images as Titchener was in affirming it.

In the first, and typical, experiment of this genre, a subject was asked to lift two weights and to judge which was heavier. What was surprising was that the subject had no notion of how the judgment was made. To be sure, there were plenty of experiences and sensations, but the judgment itself was not heralded by a sequence of coherent steps. Rather, the judgment just seemed to pop into the mind, as the subject said, full-blown and unguided by conscious processing. In a second experiment, this time conducted with psychologist E. Durr as the subject, the following question was asked, "Is this correct; 'The future is just

as much a condition of the present as of the past'?" Durr answered "No," in ten seconds. The interesting thing is that while Durr claimed to be aware of how he worked out his answer to this question, this 'working-out' was conducted neither in images nor in words. Durr's report follows:

First I thought: that sounds like something correct (without words). Then I made the attempt to represent it to myself. The thought came to me: Men are determined by thoughts of the future. Then, however, immediately the thought: that the thought of the future should not be confounded with the future itself; that such confusions, however, constitute a frequent dodge in philosophical thought. (Of words and images there was throughout no trace.) Thereupon the answer: No.¹

The imageless thought debate was never resolved. In fact, the Titchenerians and the Wurzburg psychologists were reduced to an exchange of dogmatisms--each group consulted its respective introspections and begged to differ with the other.

How can the discrepant reports of these two groups of researchers be accounted for? In part, it may be possible to appeal to the irreducible notion of 'individual differences'. We know that subjects differ widely in the vividness of the images which they find in introspection; that is, in the degree to which their images are subjectively like genuine perceptions. These differences, first reported by Galton

¹E. Durr, reported in George Humphrey, Thinking: An Introduction to Its Experimental Psychology (New York: John Wiley and Sons Inc., 1963), p. 58.

(1880),¹ were quantified by Betts² in terms of a 7-point scale.

Titchener was certainly a vivid imager; perhaps his opponents simply were not. But this is surely not the whole story here. There seems to be another factor at work. Titchener and his students were looking for images and expected to find them; the Wurzburgers were not. This must have set up very powerful demand characteristics for each group of subjects. And there is good reason to suppose that such effects would be most powerful where the overtly required performance is itself ill-defined and left more or less up to the subject, as, for example, in the Wurzburg experiments.

The possibility of importing demand characteristics into an experimental procedure involving introspective responses is, I believe, genuine. And it is a real problem--one which affects many of the contemporary investigations of image phenomena as well as those of the early introspectionist psychologists. I cite one example of this. Psychologist Stephen Kosslyn has conducted experiments designed, so he says, "to investigate the idea that images are limited in spatial extent, and that the maximal subjective size of images is constrained by the spatial medium that supports imagery representations."³

¹Francis Galton, "Statistics of Mental Imagery," Mind, vol. 5, 1880, pp. 301-318.

²G.H. Betts, "The Distribution and Functions of Mental Imagery," Columbia University Contributions to Education Series, vol. 26, 1909, 1-99.

³Stephen Kosslyn, 1980, op. cit., p. 73.

In these experiments, subjects were asked to image an animal, such as a rabbit, as if it were next to an appropriately scaled elephant or fly. The subjects reported later that the elephant "took up most of the room," leaving only a little for a tiny image of a rabbit; the fly, by contrast, took up relatively little room, leaving plenty for imaging a seemingly large rabbit. Also, these subjects required more time to see properties (such as, the nose) of an imaged rabbit when it was next to an elephant than when it was next to a fly, reportedly because properties were harder to see on subjectively smaller images. This finding was reversed when the subjects were asked to image the fly huge and the elephant tiny. Kosslyn concluded "... these introspections suggest that the medium in which one forms visual images is spatially bounded; if it were not, the rabbit could have been the same subjective size independent of the relative size of the adjacent image."¹

It seems to me, that in this experimental setting, there is plenty of opportunity for the subjects to infer just what it is that the experimenter is testing for, and hence, just what it is that he wants them to say. Kosslyn asks his subjects to describe how the rabbit looks relative to the elephant, given that they are to be pictured in appropriate scale. Anyone, even a small child, knows that beside an elephant, a rabbit looks very small indeed. And we do not need to refer to pictures of these objects in order to determine this. As for the subjects' complaint that there was "little room" for the rabbit once the elephant was imaged, such language could well be suggested to the

¹Ibid.

subjects by the experimenter's own use of expressions such as the mind's eye, mind's space, etc. They might have inferred that, for the purpose of this experiment, the mind is to be treated as a projection screen on which elephants, flies, and rabbits are to be projected and the mind's eye, as a projector. Equally, it is not surprising that the subjects would take longer to identify and describe parts of smaller animals--when the experimenter says, "Are the rabbit's eyes open?" and then, "Is the elephant's trunk curled?", the subjects will be expecting that the eye, since it is on a very small animal, occupying a very small space on the projection screen, will be hard to find and hard to focus on clearly. An elephant's trunk, by comparison, practically takes up the whole screen and is easy to see and describe. In this experiment, the experimenter's questions, and the language he uses to ask these questions, give the subjects a more than adequate basis for inferring experiment demands.

Now, it is a significant feature of Roger Shepard's experimental findings that, in addition to the linear reaction-time pattern, the subjects consistently report image rotation. In light of the demand characteristic problem, we must consider just what weight these reports ought to be given. Are they genuinely observational or are they, like the reports of Stephen Kosslyn's subjects, very much a function of the subjects' inferences about the experimenter's expectations?

A number of psychologists¹ have taken the view that Shepard's

¹Namely, Charles L. Richman, David B. Mitchell, and J. Steven Reznick ("The Demands of Mental Travel: Demand Characteristics of Mental Imagery Experiments," The Behavioral and Brain Sciences, vol. 2, 1979, pp. 264-265), Zenon Pylyshyn ("Imagery Theory: Not Mysterious--

subjects have probably inferred that the experiments have to do with rotation and so have come to report the appropriate kind of experience. This view seems to be held almost as a matter of course in respect of any and all experimental procedures which investigate imagery on the basis of introspective reports and is not, so far as I can tell, mediated by any significant degree of careful study of the details of many of these experiments, and certainly not at all, of Shepard's. This is unfortunate, as I believe the peculiarities of Shepard's experimental design guard effectively against the possible importation of demand characteristics into the experimental procedure and so preserve the observational value of these reports. These design considerations bear closer scrutiny.

Broadly speaking, Shepard's experimental mandate is to obtain information about the functional qualities (if any) of visual imagery. The obvious way to fulfill this mandate is to design a problem task--a spatial problem task, which will encourage the utilization of imagery in some capacity. As investigators, we might design problem tasks for instance, which require the assessment of the spatial extent of specified image objects, on the assumption that there ought to be a proportionate relation between the distance involved in the spatial assessment and the length of time taken by the subjects to complete this assessment. There ought to be, in other words, a fairly precise correlation between the distance factor and reaction time.

Just Wrong," *ibid.*, pp. 561-563), and Peter W. Sheehan ("Metaphor Versus Reality in the Understanding of Imagery: The Path From Function to Structure," *ibid.*, pp. 567-568).

In theory, we could design an experiment to test for this in a number of ways. We could, for instance, proceed quite informally by simply requiring subjects to picture the map of Canada and ask them to mentally trace the outline of the map, signalling in some fashion when they have completed the task. Then, for contrast, we could ask them to do the same thing in respect of the map of Nova Scotia. The point of this exercise would be to compare the reaction times for the two tasks. If the functional system of representation in this circumstance is imagistic then, on our hypothesis, relative distances must be preserved and the reaction times must be proportionate to the distance involved in the outlining task. It should, that is to say, take longer to perform the mental outlining of Canada than Nova Scotia.

This is, however, a very rough and ready measure of the spatial-temporal correlation inferred in our hypothesis, and needless to say, the experiment is much too informal to stand as any kind of proof or disproof of the kind of cognitive representation operative in this circumstance. Suppose, for instance, it was discovered that the subjects' reaction times were not proportionately related to the distance factor. Suppose even that it took the subjects longer to complete the outlining of Nova Scotia than the outlining of Canada. Would this prove that the operative cognitive representation here was not imagistic? Not necessarily--it could well be that in order to simplify the latter task, the subjects did not bother to preserve the relative size distinction between Canada as a whole and Nova Scotia in their minds and so used an 'inflated' image of Nova Scotia, one the same size perhaps, or one larger than Canada for their representation. After all, this would not be an unusual thing to do--most people who are knowledgeable about the

geography of Canada (and certainly all native Canadians) are accustomed to studying full-scale maps of the individual provinces and so would have a natural predisposition to the simplification of the problem in this way. And even if we attempt to prevent this situation from developing by instructing the subjects to use the same image for both tasks or we explain to them that it is important to maintain the relative size distinction of each map, we can never be sure that they have fully understood our instructions and are in fact complying with them. And, as a further complicating factor, if we do this, we run the risk of 'over-specifying' the task for the subjects--conveying tacitly the message that the point of the exercise is to get the timing 'right'.

Consider the scenario from the point of view of the subjects. They are instructed to mentally outline two objects, and they are told that they must use a representation or representations which preserve/s the size ratio between these objects. They know from their basic geography that these objects are vastly different in size and they know as a matter of physics, that if the relative size is preserved as they have been told it must be, it must take longer to outline the larger object than the smaller object. Might they not, therefore, consciously or even possibly, unconsciously, perform their outlining task in such a way that their expectations (and our expectations) as to timing will be fulfilled? Surely it is conceivable that given subjects functioning with this expectation might take their time performing the first outlining task but go rather quickly about the second. The point is that if we give too much information by way of instructions, we create expectations in the subjects which affect the outcome of the performance. In any case, as we have no way of monitoring the speed of each outlining

performance in each subject--we do not know whether such demand characteristics are operative. And even the subjects themselves may not be entirely in control of their respective outlining speeds. They may, for instance, simply be slow about outlining Canada because the task is new to them and then pick up speed and proficiency the second time around. Or just as plausibly, given subjects may be more conversant with the basic geography of one or other of the maps and so perform the outlining of that map with greater speed and proficiency.

Thus the problem with the map outlining experiment is that it fails to place adequate constraints on the information processing procedures undertaken by the subjects. There is simply too much opportunity for the tacit beliefs, information and experience of the subjects to influence and even interfere with the process and outcome of the cognition. Things like given subjects' knowledge of geography--their desire to perform 'correctly', to please the experimenter etcetera, get in the way of precise measurement of the supposed spatial-temporal correlation.

Suppose, then, that we attempt to devise an alternative experiment --one which provides a control against the development of tacit expectations of the experimental demands in the subjects. How can this be done? It can be done by implementing two strategies: first by simply refraining from over-specifying the task for the subjects and, in so doing, causing them to infer the point of the exercise. And it can be done by requiring the subjects to solve a test problem which is de novo with respect to the subjects' previously acquired fund of knowledge. A problem, that is, in respect of which the subjects entertain no particular expectations simply because they have no clear basis of information under the guidance of which they would be able or

prepared to make inferences. Take a case in point. Suppose that we ask a group of subjects to tell us how many windows there are in their respective houses and then time the interval which elapses between question and answer. If the cognitive representation which is operative here is imagistic--which is to say that it conveys information by depiction rather than description then the answer must have been arrived at incrementally--by way of a step-by-step assessment of the spatial extent of the four sides of the house, counting windows 'along the way'--just as would be the case if the subjects were asked the question when the houses are physically before them. This means, then, that the length of the subjects' reaction times ought to be a function of the number of windows counted. Clearly the virtue of this kind of problem is that with a few notable exceptions,¹ people simply do not know how many windows there are in their houses and none of the information regarding the structure of their houses which they might well have ready to hand, is adequate as an inference base. They might know certain facts about their houses--that they are large or small, that they are 'Cape Cod' or some other style, but these facts will furnish only hints as to whether, relatively speaking, the house has many or few windows but nothing more precise than this. Thus the problem forces the 'cognitive' hand (if I may be permitted to mix metaphors) of the subjects--it necessitates a kind of on-the-spot calculation of quantity

¹I have in mind people such as carpenters, engineers, fanatical housewives or someone who has just replaced the windows in his house. Such categories of people could presumably be culled from the test group prior to the experiment.

over space.

However, there are certain defects inherent in this experiment, which makes it less than desirable as a test for our hypothesis. For one thing, though all the subjects are working on the same problem not all of them and more than likely, not any two of them will be working in respect of the same problem object. Each subject will be making his/her decision regarding quantity of windows by thinking of his/her own dwelling place. This makes for certain complications when it comes to interpreting the data generated by this experiment.

Suppose, as a hypothetical case, that we find that there is no clear correlation between the subjects' reaction times and the quantity of windows they report after their deliberations. Some of the subjects report a very few windows (say less than 10), but yet exhibit lengthy reaction times, while other subjects report many windows (over 10), but come to a decision in the matter much more quickly. What would this prove? Would it prove that the kind of cognitive representation operative in this circumstance is not imagistic? Not at all, for there is an alternative explanation available here--an explanation which has to do with the differences in the size of the houses considered, rather than the nature of the representation which has been cognitively operative. It is quite conceivable, for instance, that in the case of an individual who reports few windows but has a disproportionately lengthy reaction time that that individual's house is very large such that it takes him quite some time to scan a representation of the house counting windows as he 'goes along'.

In other words, in such circumstances, an individual could well be processing information by way of an image representation but because of the unusual structure of his house (large with only a few windows) and, accordingly, of his representation of it, he displays a reaction time which is equally unusual and, in this case, entirely unexpected.

The point is that this experiment tests for the correlation of reaction time to quantity of windows but ignores what is just as much a crucial factor--the area which must be covered during the process of counting. If that area is large, then even if the number of windows to be counted is small, there ought to be an increase in reaction time to account for this. For in an imagistic circumstance, where relative distances are preserved, it is not just quantity of windows counted which produces the reaction-time effect, but quantity over space.

The basic flaw in this experiment is clearly that it fails to provide some kind of external control on the object in respect of which the subjects will be conducting their problem solving activities. Because they all use different objects, it is never entirely clear how we ought to interpret the data. There is always the variable relating to the structure of the house--in particular relating to the ratio of the number of windows to space which complicates the issue. And even if the data would seem to confirm our image hypothesis, we can never be sure--such results could be indicative of the operation of the structure variable rather than the nature of the kind of cognitive representation used. The remedy for this situation is obviously to stipulate a single test object for the subjects--preferably a test object which is easy to visualize, but one in respect of which the subjects have had no previous

experience and so entertain no expectations one way or the other.¹

Now Shepard's particular version of the reaction-time experiment is distinguished by the fact that the subjects are not permitted to conduct their spatial calculations in the abstract, in respect of objects which they must conjure from memory. On the contrary, a test stimulus is provided for them. This stimulus is comprised of objects which have been specially designed by Shepard. The objects are presented to the subjects in the form of perspective drawings and they are in pairs. At first sight, the objects in a given pair appear to be more or less the same, but it is difficult for the subjects to tell for certain, as the objects are presented in different 'orientations'. (Object X in a given pair may appear to be standing 'straight up', so to speak, while object "Y" may appear to be on its side, relative to 'X'.) The subjects are required to indicate by pressing the appropriate lever whether or not the objects are the same shape.

¹It is worthy of note that this experiment has actually been undertaken by two psychologists--P.R. Meudell and later by W. Janssen. They have published their results, respectively, in "Retrieval and Representation in Long-Term Memory" (*Psychonomic Science*, vol. 23, 1971, pp. 295-269 and in On The Nature of The Mental Image. Soesterberg: Institute for Research on Perception, 1976). Both researchers report the following: that the imagery hypothesis has been confirmed--specifically that the time taken by the subjects to answer the question varied in a linear fashion with the number of windows counted and that virtually all the subjects when questioned after the fact claimed that they solved the problem by imagining themselves moving around the house, visualizing and counting the windows. For the reasons I have given above, I do not think that this experiment is capable of providing a clear confirmation of the hypothesis that the subjects were solving their problems by visualizing themselves counting windows on imaged houses.

A principal variable which Shepard seeks to record here is the subjects' reaction times. The reason for this is obvious: If, as per our hypothesis, the subjects' calculations are going forward within the context of a system of analogue or imagistic representation, then the decision as to shape similarity must be made on the basis of a rotation of one image structure into congruence with the other. For in the analogue format, problem solving is a wholistic-visual¹ affair. Accordingly, the time required to reach a decision should be in proportion to the degree to which one image must be rotated in order to achieve a congruence or fit with the other. Thus the greater the discrepancy in angle of rotation between the orientations of the two objects, the longer it must take the subjects to perform the appropriate image manipulation and record their responses.

The significance of Shepard's provision of an external test stimulus is clearly that it serves to constrain the subjects' cognitive operations. For instance, by requiring the subjects to focus their attention on a single stimulus, Shepard successfully eliminated the 'object variance' factor which was likely to be significant in the

¹I use the adjectives 'wholistic-visual' here by contrast to 'analytic-linguistic'. The sort of problem solving exercise suggested by these latter adjectives would be a descriptive one and would, presumably, involve the generation of linguistic descriptions of the parts and the proportions of the parts of each of these objects with the eventual comparison of separately generated descriptions. This would, of course, not be the case in the analogue format, where, presumably, such a problem must be worked-out literally--in spatial terms such that, in some sense, the solution would 'unfold', 'take place' before the mind's eye of the subjects.

experiments considered above. And having eliminated this, Shepard is free to interpret his reaction-time data as clearly pointing or not pointing (as the case may be) to an image form of cognitive representation and to analogue problem solving. Not only this, but the very fact of the presence of a stipulated test stimulus ensures a greater degree of precision in the measurement of reaction time. It will be recalled that in the 'map outlining' experiment, the reaction time is clocked as the interval which elapses between the assignment of the test problem and the subjects' verbal response. However, on this procedure, it is never entirely clear when the individual subjects actually begin the calculation process--obviously it is quite conceivable that some subjects are still assimilating the instructions long after others have already moved to the solution process. On Shepard's procedure, this contingency need not arise, the problem can be explained to the satisfaction of all the subjects prior to the presentation of the test stimuli, such that with the onset of the stimulus, the clock can start, so to speak, with regard to the mental calculation. And it must not be forgotten that memory can also affect reaction time. If as in the former two experiments, subjects are required to furnish from memory the details of the object of concern, it can be expected that some individuals will have less clear memory than others and so will still be engaged in the process of sorting out and making decisions on detail when other individuals, with, say, instant recall, are otherwise engaged.

The particular design of Shepard's test objects is obviously significant as a controlling factor in his experiment. The objects in question consist of cubical blocks attached face-to-face to form a

rigid asymmetrical structure with two free ends and two or three right-angled bends. In the experiment proper, these objects are presented in the form of drawings or caricatures in which three-dimensional perspective has been preserved. Obviously, Shepard's idea here is to design objects that are easy to visualize but hard to describe verbally. If he did not use such objects he would run the risk of failing to induce his subjects to use visual imagery or of failing altogether because he has demanded too much from their powers of imagination. Not only this, but these objects, by virtue of their unusual design, are such that the subjects will never have experienced anything like them before. This being so, the subjects will not have any preconceived notions or expectations as to the outcome of the experiment.¹ They are simply confronted with new material and they react accordingly. Thus the novelty of the objects is a valuable feature in that it neutralizes the possibility that the subjects' expectations will interfere with the process of calculation and modify the experimental results.

But there is a further, more intriguing possibility presented by the so-called 'novelty factor' in Shepard's experiment. I have argued that because of the unfamiliarity of the objects, the subjects are unlikely to develop any particular expectations as to the outcome of the experiment. They will not be thinking that they ought to perform one way or the other and so come to fulfill this expectation either

¹It will be recalled that in the 'map outlining' experiment, the subjects might have developed expectations as to the timing of their responses on the basis of their understanding of the relative size of Canada and Nova Scotia and their (reasonable) inference that it ought to take longer to outline the former than the latter.

consciously or unconsciously. They simply have no 'stake', so to speak, in the experiment results. As a correlative to this, I would like to suggest that neither will the subjects have any particular conviction as to strategy--that is to say they will have no preconceived notions as to how the process of calculation ought to go forward. This is by contrast to the situation in the 'window assessment' experiment, where subjects are quite familiar with the object of concern and they are accustomed to resolving factual issues regarding the structure of this object by 'taking a look' and, if need be, by walking around and counting features. They have, in other words, a strategy already worked out in respect of this object. It is reasonable to assume, therefore, that the subjects will have a natural predisposition to attempt some such strategy in the mental sphere. The strategy is, after all, in the day-to-day practical circumstance in which painters or builders operate very successful. But in the case of Shepard's objects, the subjects have no strategy ready-to-hand--they could not because they have no opportunity to develop one.

Now suppose that we find that in addition to the reaction times of the subjects correlating neatly with the rotational discrepancy of the test objects (as on our image rotation hypothesis they are supposed to) they are consistent throughout the entire experimental sequence. In other words, it takes the subjects no longer to solve a problem requiring a 60° rotation at the beginning of the experiment, than it does at the end. Would this not tend to point not only to the existence of an image rotation but to the automaticity of the problem solving strategy? I think that it would. After all, the subjects have been presented with

objects--of which they know absolutely nothing and yet they take no time at the outset of the experiment to work out a calculation strategy, but proceed without hesitation¹--performing as quickly and as proficiently at the beginning as at the end.

Now I am not saying that the automaticity of the image representation system has been demonstrated in and through Shepard's experiments. Shepard himself does not explicitly make this inference and he does not give details as to the consistency of the subjects' reaction times (relative to given rotational discrepancies) over the course of the experimental sequence which would make such an inference possible. Indeed, it could well be that as a matter of empirical fact, individual subjects do not exhibit consistent reaction times--they may well be quicker at the end of the experiment than they are at the beginning. I simply do not know.

What I am saying, however, is that without the novelty factor being present, there would be no chance to infer anything whatsoever about the automaticity of the image representation system. Suppose, that in the case of the 'window assessment' experiment, we establish the consistency of the individual subjects' reaction times over the entire experimental sequence. This would entitle us to say absolutely nothing about the automaticity issue. For it is, as I have already argued, entirely to

¹One could expect, I think, that if the subjects were having to address the issue of strategy--deciding, that is, how to go about solving the problem--that their reaction times at the outset of the experimental sequence relative to a given rotation disparity would be longer than at a later time of the experiment simply because later they are able to mechanically apply whatever strategy they have worked out.

be expected that people would move without hesitation to the solution of the problem--they have a strategy (good, bad, or indifferent from a scientific or philosophical point of view) already worked out for this. But when people do this in circumstances, such as in Shepard's experiments, where we know there is no way that such a strategy could have been worked out in advance, then this is significant--and we begin to have reason to make certain inferences--in particular the inference that somehow it can be brought into operation, without the explicit cognition of the subject, but automatically. (See Appendix C.)

There is one further aspect of Shepard's experimental design which is worthy of note in connection with the demand characteristic problem. Shepard's solicitation of the subjects' impressions of how they solved the shape assessment problem is not, so far as the individual subjects themselves can tell, strictly speaking part of the formal rotation experiments. The subjects are simply required to answer the question--"Are objects 'X' and 'Y' the same or different?"--as quickly as they can. And their overt responses are mechanical--they press a left or right-hand lever indicating 'yes' or 'no' (these objects are/are not the same shape). It is the timing of their responses which appears to be crucial here. Introspective reports are solicited after all the experiments have been completed and on a fairly casual basis. This casualness, of course, is very desirable. For the subjects simply have no opportunity to develop a clear sense that what they say counts for anything--that it is the calculation strategy that is being tested as well as the correctness and/or speed of their responses. Consequently, there ought to be a strong presumption of sincerity on the part of these subjects in

regard to their reports of image rotation.

3.5 Conclusion

In this chapter, I have argued that the image rotation phenomenon is best conceived on the identity model--as an experienced brain process. In addition to this, I have attempted to work out, in schematic terms, just what image rotation in the brain might amount to. In order to accomplish this, I have made use of an analogy to computer operation. I have also argued that certain important methodological consequences follow from this view of image rotation--specifically, that cognitive investigators ought to treat introspective reports of such phenomena as genuinely observational. The process of image rotation is not just theoretical--something postulated for the sake of getting the experimental process started--but is a genuine problem solving strategy and ought to be treated as such. Finally, I have argued that specific aspects of Shepard's experimental design counter the possible incursion of demand characteristics into the experimental process. Shepard's subjects report image rotation during the spatial problem solving sequences because this is how they experience the problem solving activity of their brains.

CHAPTER 4

Spatial Problem Solving With Language: The Alternative Strategies

4.1 Introduction

Several psychologists and philosopher Daniel Dennett have sought to discredit the theory of mental rotation by devising alternative problem solving strategies which, they argue, Shepard's subjects could have used in order to calculate the shape similarity of the test objects and which would have produced the required reaction-time effect. Viewed collectively, these alternative strategies are organized around the belief that the basic unit of cognitive representation in the case of spatial problem solving is linguistic rather than imagistic and that instead of performing a mental rotation, the subjects are really just describing the test objects to themselves.

Four different strategies of spatial problem solving by linguistic means have emerged in the literature. I have provided names for each of them. In order of their treatment in this chapter they are: the wholistic description strategy, the feature-by-feature description strategy, the rotation by propositional increments strategy, and the tacit knowledge strategy.

In this chapter, each of these strategies for spatial problem solving will be discussed in detail and each will be considered as a possible rival to Roger Shepard's mental rotation strategy. In this regard, I shall be concerned to look for two attributes: first of all

and most basically, a rival strategy must provide an efficient means of resolving the shape similarity/dissimilarity problem and, secondly, it must do so in a way that is bound to produce the linear reaction-time effect. For, clearly, a genuine rival must be seen to generate the reaction times which people consistently exhibit when engaged in Shepard-type problem solving.

4.2 The Wholistic Description Strategy

The wholistic description strategy has been proposed by psychologist John R. Anderson. Anderson argues that the reaction-time pattern exhibited by Shepard's subjects can be as well explained on the assumption of linguistic problem solving as on the assumption of mental rotation. All that is required, says Anderson, is the production of a description of each of the test objects with particular attention to the orientation of each object in space. Anderson gives the bare bones of his theory in the following passage:

One of the most influential phenomena uncovered in recent research in cognitive psychology has been that of mental rotation. The basic finding is that the time to decide that one object is a rotation of another object is a monotonic and often linear function of the amount (degrees) of rotation. This is taken as evidence that subjects mentally rotate an image of one object of the pair into congruence with the other.... It is a simple matter to propose a propositional model that mimics this image model. The model would involve a propositional description of an object and of its orientation in space.¹

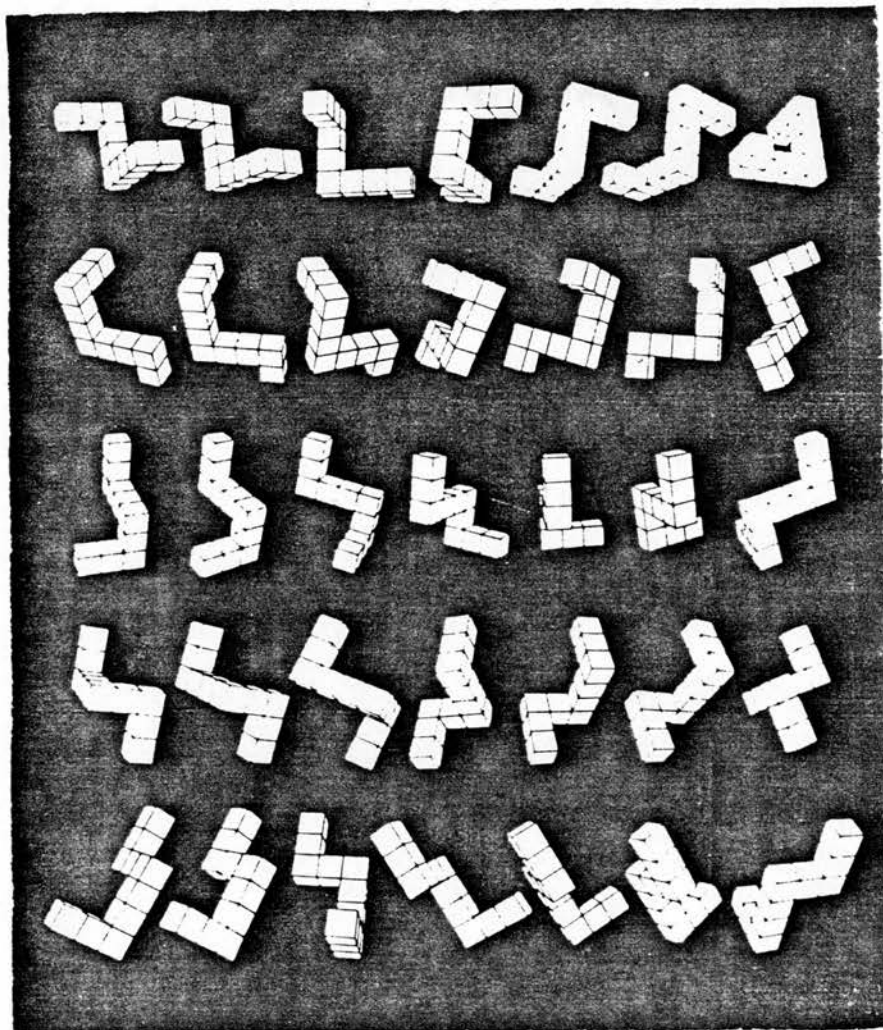
¹John R. Anderson, "Arguments Concerning Representations for Mental Imagery," Psychological Review, vol. 85, 1978, p. 200.

Just what has Anderson in mind here? Presumably, Anderson intends that each of the test objects in a given pair should be treated as a separate unit and that detailed descriptions of each of these units should be generated in turn. And, ultimately, these descriptions should be compared for structural similarity. This generation of descriptions is easily accomplished, of course, if the objects are of very simple construction, or, even better, if they are of some readily recognizable shape (as in the hypothetical case discussed earlier where the objects to be described were an A and A on its side - \succ - respectively) such that the subjects already have a descriptive vocabulary ready to hand.

But now consider the specifics of generating descriptions for the objects used in Shepard's experiments. The subjects taking part in these experiments are presented with perspective drawings of unusual geometric shapes. (See Figure 7.) The objects are of complicated design. They are objects comprised of ten blocks. There are 'sections' or 'arms' projecting in various directions from each of these objects, and these arms have a right angle bend in them. As is the case with the arms themselves, the bends appear to occur according to no particular pattern or rule--their position along the respective arms appears, at first glance, to be haphazard. Obviously, accuracy and detail count here. For a candidate description must be specific enough to make possible an informed comparison--one structural feature omitted or misdescribed could negatively influence the outcome of the comparison.

Now it would seem that ordinary language is not a very effective vehicle for the specification of these rather fine spatial relationships. In fact, a 'proper' descriptive account (i.e., one which is

Figure 7



Reproduced from R. Shepard and L. Cooper, 1982, *op. cit.*, p. 24.

sufficiently detailed) in this language of any one of Shepard's objects would probably be exceedingly long. Presumably, at the least, the 'arms' would have to be given names of some sort (they could be called 'A', 'B' and 'C', for instance) and their respective locations on the

'body' of the object and the particular directions in which they project, specified (say, arm 'A' is at the top right-hand side and is so many degrees to the north-east of arm 'B'). Bends would require specification and naming as well--specification, presumably, in terms of where they occur on the arm and the number of blocks within the entire structure would have to be specified.

Needless to say, this sort of descriptive process would be very time-consuming--one serious mark against it as far as efficiency goes. After all, the subject must make a series of on the spot decisions about how to describe the object--names for arms on an object of a kind never before encountered do not just come to mind as part of our ordinary language vocabulary. They must be devised and applied consistently. (Obviously it would be a mistake to use letters to name arms in one object and numbers to name arms on the other object.) Indeed, the whole idea of a distinction between 'arm' and 'body' which is imported into the description by the utilization of the concept 'arm' is ambiguous. How do we decide what constitutes 'arm' and what constitutes 'body' in these objects? On the other hand, do we have a more suitable ordinary language descriptive terminology to substitute for arm? What about the term I used earlier--'section'? Is this any less ambiguous than arm? Is it really likely that someone upon hearing a description couched in terms of sections would be able to develop a clear idea of the kind of geometric structures with which Shepard is working?

And what about the storage capacity of the immediate memory? Would it not be overtaxed to an incredible extent if we suppose this kind of descriptive language? It seems hardly likely that the subjects would be

able to bear in mind and manipulate two such lengthy descriptions during the final comparison process.

This is not to say that ordinary language does not have a distinct spatial vocabulary. There is such a vocabulary (it contains concepts such as below, above, to the right of, large, small, etcetera) and it is perfectly adequate for the task of describing the world for ordinary purposes. For in the ordinary course of things--when we are teaching someone the rudiments of driving a car, or when cautioning a companion to take care when descending a staircase--all we need to do is judge and describe distance and objects more or less. It is speed and simplicity which count here and too much detail just gets in the way. We may note to the student driver, for instance, that the car must be steered 'a little more to the left' (in Canada) if it is to avoid hitting the parked car 'along the side', or we note to our companion on descending a staircase that the last step is 'lower than the rest' so that he might adjust his stride appropriately. The rough-and-ready quality of this vocabulary is all well and good in the context of our day-to-day dealings with the world but it is inadequate if we need to describe certain unusual objects in space in any detail.

Of course, the inadequacies of our ordinary language spatial vocabulary could be compensated for on the supposition that people utilize a technical language of spatial description in their problem solving. We know, for instance, that all geometrical spaces can be precisely represented in the language of analytic geometry. For example, in this language, the equation $X = Y$ designates a straight line which bisects the X and Y axes in Cartesian coordinate space.

Owen Flanagan¹ has postulated this as the "possibility proof", as he calls it, that people could conceivably function by analyzing the Shepard figures and representing their spatial properties in the language of analytic geometry. He argues that people could then "compute congruence ... by manipulating sentences."

But, I must ask, is this a plausible supposition? Certainly it is true that Shepard's unusual objects could be represented precisely and adequately in the language of analytic geometry. But how many people is it reasonable to suppose are actually able to do this? The subjects who have participated in Shepard's rotation experiments are a random sample of university students, selected, for the most part, out of courses in introductory and advanced psychology. Can we suppose that all of these students or, indeed, that any of them, are sufficiently conversant with the niceties of analytic geometry such that they might undertake the descriptive exercise Flanagan has envisioned? And if this is what the subjects do, then it is surely very odd that not one of them has reported that this is how they undertake to solve the test problems. On the contrary, the fact of the matter is that the subjects consistently report the mental rotation strategy of problem solving.

And are we to infer that individuals (like myself for instance) who have nothing more than the most rudimentary knowledge of formal geometry are, by necessity, excluded from the possibility of solving the spatial problem. If we adopt Flanagan's supposition of problem solving by way of the language of analytic geometry, then surely we must

¹Owen J. Flanagan, Jr., 1984, op. cit., p. 191.

infer something of this nature. This will be a surprise to many people - myself included - who have no difficulty solving the problems but who know next to nothing about describing objects in the language of analytic geometry.

Flanagan's supposition fails, then, because it presupposes a knowledge and skill which in point of fact very few of us have. It is one thing to say that in principle people could utilize a given language in order to solve their spatial problems, but quite another to provide a plausible argument demonstrating that this is in fact what they do. In this connection, it is interesting to note that within the fields of mechanical and civil engineering, the chosen medium for conveying spatial information about unusual shapes (such as machine parts) is the diagram. This is a striking practical endorsement of the efficiency of analogue - as compared to - linguistic representations of spatial information.

To take a specific instance of this, in mining operations, it is sometimes necessary to join two tunnels. Obviously, in physical terms, this will be achieved in either one of two ways: by constructing a third 'connecting' tunnel between the original two or by extending one or both of the original tunnels in an appropriate direction at an appropriate angle so as to bring the tunnels to a point of intersection. The engineer's problem is to calculate the absolute position of the tunnels relative to one another and this requires that he generate representations of each of the tunnels in three dimensions. The engineer solves this problem by drawing sketches of each tunnel from the perspective of three views--specifically--the 'top', 'front' and 'side' views.

This is essentially an imagistic process for the engineer. For example, in order to draw the front view of a tunnel, the engineer visualizes the tunnel as if it were not surrounded by earth, but free-floating, so to speak, and visualizes himself directly in front of the tunnel so that he can look 'squarely' at it--his line of vision being at right angle to the tunnel structure. Likewise, the engineer draws the top view by visualizing himself standing directly over the tunnel at right angle to the earth surface looking down at it. And he draws the side view by visualizing himself standing to the right or left of the tunnel. Obviously, once the relative three-dimensional positions of the tunnels is established, the determination of how best to achieve the joining is quite straightforward--indeed, it is as if the tunnels were no longer out of sight beneath ground, but structures physically present to the engineer.

Apparently, a trained engineer can generate this kind of three-dimensional representation with a fair degree of speed and with a minimum of effort. By contrast, a 'descriptive' solution to this problem of specifying the relative locations of the tunnels would be unbelievably complicated and quite possibly unachievable.¹ And in any case, engineering graphics is the chosen method of dealing with these kinds of design problems and, routinely, aspiring engineers receive a minimum of a full term's training in this. A standard text in the subject, describes the nature of this training in these terms:

¹This has been argued by J.H. Earle in the introduction to the textbook, Engineering Design Graphics (Massachusetts: Addison-Wesley Publishing Company, 1983).

The student of engineering requires training both in visualizing the appearance of structures and in representing the exact shape of structures by means of drawings. Such training may be acquired by studying the geometry of engineering drawing, and by solving problems. The student of this subject obtains very valuable training in thinking in terms of three-dimensions.¹

There is one further point that I want to make about the wholistic description strategy. As I have indicated earlier, a strategy which is a genuine rival to mental rotation must be able to produce the linear reaction-time effect as well as provide a means of solving the problem at hand. Obviously, Anderson and Flanagan believe that the strategies they have proposed can accomplish this former task (otherwise there would be no point in their proposing these strategies in the first place). However, of the two, Flanagan alone makes an attempt to explain how the linear reaction-time pattern might come about. In a footnote to his discussion of problem solving in the language of analytic geometry, Flanagan suggests that it may be the case that the number of propositions required to describe each of the objects in a given test pair will increase linearly with the degree of orientation disparity of the objects and that, consistently with this, computation time over them will increase linearly as well.²

¹George J. Hood, Geometry of Engineering Drawing (York, P.A.: The Maple Leaf Press Company, 1926), p. 2.

²Owen Flanagan, 1984, op. cit., p. 310.

Frankly, I do not follow Flanagan's reasoning here. On the supposition that people are generating linguistic descriptions of the test objects in turn and then comparing these descriptions, reaction times should be a function of the complexity of the objects being described and nothing more. Simply put, the time taken to calculate the shape similarity of an object pair involving objects with many component parts should be greater than the time taken to perform the same calculation in respect of an object pair involving objects with few component parts.

Consider a particular case. Suppose that Shepard's subjects' calculations are going forward on the basis of a system of linguistic representation (either ordinary language linguistic representations or the linguistic representations of analytic geometry). And suppose that the test pair in a given case is structurally complex, but exhibits an orientation disparity of a mere 20° . If linguistic representations are being used, and all of the component parts of the objects are being described, then it should take the subjects longer to complete the object descriptions (and, hence, longer to complete the overall similarity calculation) in this case than it would in a second case in which the subjects are presented with a pair of structurally simple objects exhibiting an orientation disparity of, say, a full 180° . In the latter case, the descriptions of the objects will be short and uncomplicated and therefore, will take less time to generate.

It will be noted that, on the wholistic description strategy, a fluctuation and variation in the subjects' reaction times can occur and that, over the course of a number of trials, this fluctuation can even

give the appearance of conforming to a linear pattern. But the pattern in question will be tied to the object complexity factor rather than to the orientation disparity factor. And thus it does not produce the genuine Shepard reaction-time effect which is required if the wholistic description strategy is to rival the mental rotation strategy.

4.2 The Feature-by-Feature Description Strategy

Anderson and Flanagan have argued that the subjects participating in Shepard's experiments represent the test objects to themselves separately and wholistically - which is to say that they generate a description of each object in a given pair in its entirety and in sequence. But there is, of course, no reason to suppose that the linguistic descriptions must be of wholistic construction. On the contrary, it is perfectly conceivable and, in fact, it has been argued by psychologists Marcel Just and Patricia Carpenter,¹ that the subjects describe the test objects to themselves on a 'feature-by-feature' basis, such that a single linguistic representation is simultaneously generated for both objects.

According to Just and Carpenter, the feature-by-feature description process occurs in the following manner: One object in the pair, (X), is scanned by the individual subjects and a significant component part of X is identified and described. The second object (Y) is scanned for the purpose of locating the corresponding part to that described

¹Marcel Just and Patricia Carpenter, "Eye Fixations and Cognitive Processes," Cognitive Psychology, vol. 8, 1976, pp. 441-480.

in X and, if found, is described. This process of feature identification and description in X and Y is continued until all the significant features have been accounted for, in which case, the objects are found to be of similar three-dimensional shape. Supposedly, it is the degree of orientation disparity between X and Y which complicates this process. The greater the disparity, the more difficult and hence more protracted will be the task of identifying the corresponding features in Y. Thus, on this particular strategy for problem solving there should be some kind of correlation between the reaction times exhibited by the subjects and the degree of orientation disparity of the test objects.

Now it is interesting to note that our practical experience in regard to matters of spatial decision-making would seem to attest to the validity of the reasoning behind the feature-by-feature strategy. It does seem, that is to say, that whenever we are attempting to locate some specific feature on a 'second object' involved in a comparison, it is of substantial assistance if that object is in strict spatial alignment with the first object. Indeed, anyone who has worked on a jigsaw puzzle will appreciate how much more quickly the piece with the odd-shaped bump on it which fits into the space in the puzzle with the odd shaped bump is recognized if that piece just happened to have fallen beside the space in question and is arranged such that the respective bumps are in spatial alignment.

Suppose, to take a particular case, that we are looking at two keys--one which we know to be our front-door key and the other which we think or suspect is the spare front-door key. In the absence of the

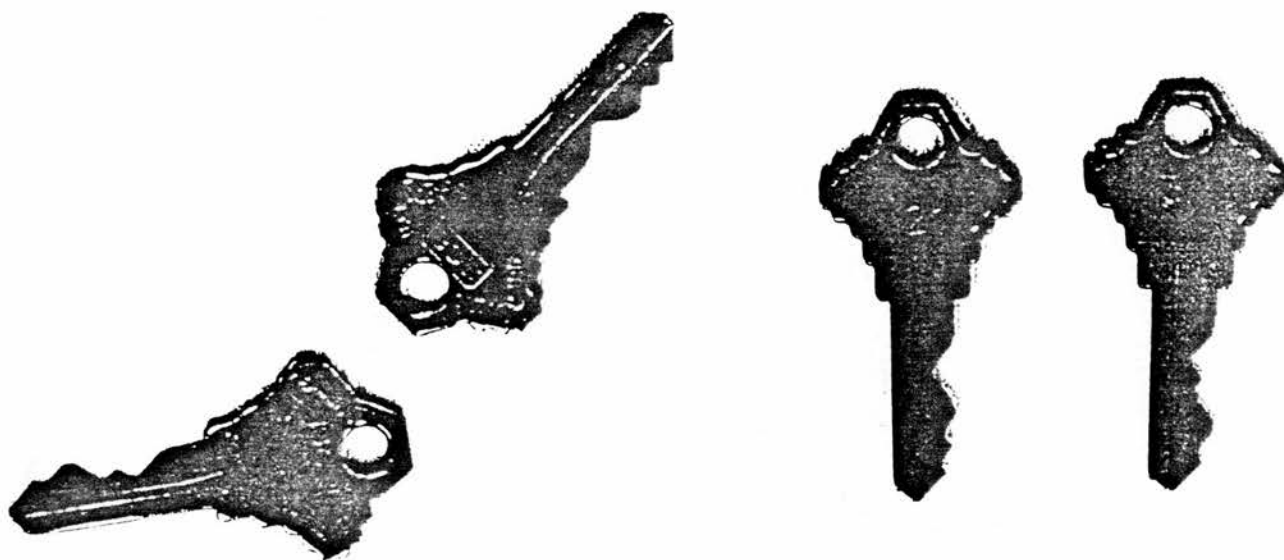
appropriate empirical test (by actually trying the supposed spare in the front-door), we might¹ decide the issue by undertaking a feature-by-feature comparison of the two keys. Now if the keys are in strict spatial alignment--if the 'top' of each key (which is to say, the part that is or can be attached to a key ring) is facing the same direction, then the task of comparison will be relatively straightforward. All we have to do is identify some structural feature (presumably a bump or a groove) on the model key and then scan across in a straight line to locate the corresponding area on the second key. If when we do this we do not find the same bump or groove then we know that the so-called second key is not a spare. If we do find the appropriate structure then we continue our feature-by-feature comparison.

On the other hand, if the keys are not in strict spatial alignment and if for some reason we are not able to move them into the desired arrangement, then the process of comparison becomes somewhat more involved. We cannot just move directly to the corresponding area on the second key, but must reconceive the situation, reasoning that since the keys are reversed or since the second key has been rotated so many degrees out of congruence with the model, the structural feature in question, should, if present, be somewhere on the left-hand (as opposed to the right-hand) side or is to be found off to the left slightly,

¹I say we might decide in this way for the sake of argument and for purposes of providing a practical illustration of the manner in which feature-by-feature description might go forward. Obviously the preferred means of resolving this problem (in the absence of the possibility of trying the key in the door) would be analogue - by physically placing the keys one over the other so as to determine the congruence of their respective bumps and grooves.

etcetera. Or, in the absence of any clear geometric sense as to where the particular bump and/or groove might be located on the second key we must resort to simply visually scanning the key to see if we can find it. There is, in other words, at least one additional step involved here--we must reconceive the corresponding areas under the guidance of some sense of where these areas ought to be, or failing that, attempt to find the feature by a process of scanning which, if we are not lucky enough to come upon the feature right away can be labourious and even require repetition. In either case, there is necessarily an increase in the time it takes for the spatial calculation to go forward.

Figure 8



Clearly, these keys are not identical and this is more obvious in Frame #2 than in Frame #1. Spatial alignment in feature-by-feature comparison counts.

Now clearly, this second strategy for spatial assessment by linguistic means has a considerable advantage over the original Anderson and Flanagan strategy. It will be recalled that in the case of the latter, the orientation factor has no causal influence on the course and outcome of the descriptive-comparative process. Descriptive representations of each object are generated separately and wholistically and it is the inherent structural complexity of the objects which produces the fluctuation (if any) in the subjects' reaction times. But if we assume that descriptions of the objects are constructed on a feature-by-feature basis, the orientation factor then becomes causally significant. For integral to the feature-by-feature strategy is what might be described as a 'search component'. Features must be located on the second object involved in the comparison and this process is made more difficult (and hence more protracted), if the objects are out of congruence with one another.

Thus it would appear that we have in the feature-by-feature strategy some competition for Shepard's image rotation strategy. There is, that is to say, reason on the new strategy to assume that there will be some kind of correlation between reaction time and the orientation factor. But, the question is, what kind of correlation will this be? It will be remembered that Shepard's experiments indicate a correlation of a very specific kind; the reaction times of the subjects increase gradually and uniformly with the degree of orientation disparity of the test stimuli. For every degree of increase in orientation disparity there is a proportionate increase in reaction time. Will this be the case if we suppose the feature-by-feature strategy? In

particular, does the degree of difficulty in recognizing and locating specific features on the second object necessarily increase in proportion to the degree of orientation disparity?

This, it seems to me, is the real test of this strategy. If there is not, or not always, an increase in the difficulty of the 'recognizing and locating' task, then there is no reason to suppose that the reaction-time effect demonstrated by Shepard in his experiments can be replicated on the supposition of feature-by-feature problem solving. And if this is so, then we must conclude that such a strategy for problem solving does not constitute a convincing alternative to the image rotation strategy. After all, as I have argued above, the logic of operation of mental rotation dictates the kind of reaction-time behaviour that is in question here. An 'alternative' strategy must do the same.

Let us suppose, then, that the orientation disparity of two objects (X and Y) is a full 180° , such that each object is 'on its side' relative to the other. Is it not conceivable that in this circumstance (in respect of some object pairs at least) it would be as easy, or even easier to recognize corresponding features in X and Y than it would be to recognize the same features if the disparity between X and Y was a mere 20° or 80° ? In such a case, the absolute side-by-side presentation of the objects might well accentuate, relatively speaking, the features of the objects and so facilitate rather than hinder their comparison. In fact, if X and Y are the same shape--all the parts and relations are simply reversed (which is to say that right becomes left in one object while left becomes right in the other and top becomes bottom in one object, while bottom becomes top in the other) and we simply have to

mechanically carry-out the implications of this right-left/top-bottom reversal in order to identify the location of the features found first on X, on Y.

What I am suggesting, then, is that in the 180° disparity situation, there is less chance that the subject will be forced to undertake a 'hit and miss' calculation procedure. Provided that the subject in question recognizes that Y has been rotated 180° about the picture-plane axis away from X, he can proceed more or less systematically to calculate the location of X's features on Y. He should expect, for instance, that a given feature on the right hand side of X is to be found on the left hand side of Y and so go directly there. On the other hand, in the case of a less extensive orientation disparity--say a disparity of 80° , Y is much less dramatically placed relative to X and the subject is highly unlikely to have any clear geometric sense of where the features found on X are likely to be on Y. It will not be the case that the features on Y will simply and uniformly be in a position of reversal relative to those on X. And the subject is more or less obliged, in this circumstance, to undertake a wholesale scanning procedure, the efficiency of which (as I have noted above) is very much a function of luck. I would argue, therefore, that in this circumstance the Shepard reaction-time effect would not obtain. The subjects' feature-by-feature analysis of the 180° disparity pair will be more efficient and, therefore, require less time or, possibly, roughly the same amount of time that will be required to make the similar calculation in respect of the 80° disparity pair. This circumstance serves, in short, to falsify the prediction of Just and Carpenter that the feature-by-feature strategy

could produce the reaction-time behaviour described by Shepard.

There is another empirical circumstance which, it seems to me, would negate the ability of the feature-by-feature strategy to produce the Shepard reaction-time effect. What if the first component part the subject identified in object X is simply not to be found in object Y? Then, clearly, on the feature-by-feature strategy, the subject would conclude that X and Y are not the same virtually instantaneously--without any further investigation. And the subject's reaction time will be correspondingly instantaneous. This could well happen in a case where the orientation disparity between X and Y is considerable--say 150°. On the other hand, it could happen in a second case, that it is the fourth feature compared which is found to be missing in Y, but the orientation disparity between this X and Y pair is a mere 20°. Once again the subject would respond negatively to the test pair--but this time not instantaneously. For on the feature-by-feature analysis, the reaction time of the subject reflects the number of features compared. Clearly, in respect of these two cases there cannot be a gradual and uniform increase in reaction time in accordance with orientation disparity. On the contrary, the reaction time for the 150° disparity pair would be shorter relative to the reaction time for the 20° disparity pair. And once again, I conclude, the Shepard effect is not achieved by the alternative, feature-by-feature strategy.

4.4 The Rotation By Propositional Increments Strategy

Daniel Dennett has elaborated a third strategy for spatial problem solving in a linguistic medium. According to Dennett, during a Shepard-type problem solving sequence, there occurs a kind of private dialectic in which the subject engages with himself. The subject asks himself, for example, "What would one object 'look like' relative to its counterpart if it were turned just this much?" and then he proceeds to describe the indicated transformation. This process continues through a graduated series of questions and answers until it becomes clear to the subject whether or not there is an equivalence of shape between the pictured objects. Presumably, it is Dennett's calculation here that the more extensive the orientation disparity exhibited by the test objects, the greater the number of increments there must be in the subject's problem solving strategy. In particular, the subject must ask himself more questions in order to decide whether or not the two objects are the same shape and so must take longer to arrive at his ultimate solution. Dennett describes the process which, he argues, takes place during the problem solving sequence:

Isn't it really just that these discrete steps are discrete propositional episodes? Now it looks like this, but if I imagine it turned that much, it would look like that ... ah yes, it would eventually look just like the other one. But the flicking, you may insist, is clearly part of a motion observed--the axis of rotation is, perhaps, vertical, not horizontal. But your reason for saying this is just that your intermediate judgments define the rotation. They are judgments that fall in an order that would be the

proper order of perceptual judgments in the case of watching a real image rotate around a vertical axis.¹

It seems to me that Dennett's proposal is problematic in several respects. First of all, and perhaps most crucially, it presents a problem of interpretation. Presumably, Dennett's primary intention here has been to deny that there is or could be any such thing as a rotation going on and that there are no images in the mind or in the brain. (There is just a series of linguistic, or as Dennett describes them, 'propositional', episodes which, because they occur in an ordered sequence--a sequence which apparently follows that appropriate to an actual rotation--create the illusion of continuity.) But if this is so, then I am at a loss to know what to make of certain of his remarks in the passage cited above. Surely he has imported images and the process of rotation, for that matter, in the backdoor of this account.

In particular, I am concerned about Dennett's caricature of the calculation process which is contained in the statement: "Now it looks like this, but if I imagine it turned just that much, it would look like that" My question is this: What can this and that possibly refer to but distinct stages of the image rotation process? Indeed, what Dennett seems to be describing here is not a rival to the image hypothesis, but the image hypothesis, complicated a bit by the suggestion that we 'eavesdrop' on our brain processes, registering certain stages of these processes propositionally. We note that one object

¹Daniel Dennett, Brainstorms (Massachusetts: The MIT Press, 1981), p. 168.

looks more like the other now (that it has been rotated a few degrees) than it did before, and we continue to make such rotations until it becomes clear to us that the objects are the same shape or that they are not. But the essential process here, I take it, the process which bears the burden of the actual cognition, is still image rotation. The propositional episodes are simply accompaniments of this process.

The situation which Dennett appears to be describing, is not unlike that of a child using building blocks to construct a model airport, commenting (in this case, out loud) on the various stages of the construction. ("Now I put this red block on the green one, and it looks like a tower....") The commentary is a verbal reaction to the airport making, it is in no sense an alternative construction process, or even a constituent process. It may be useful in that it could serve to help the child focus his attention on his work or it may simply function so as to register the child's pleasure at the completion of each stage of the task; but it is clearly unnecessary. The child need not engage in the commentary in order to perform the construction, and in fact, as an adult, will almost certainly not do so.

So if this is what Dennett has in mind, then I see no difficulty for the image rotation strategy for problem solving postulated by Roger Shepard. It is not in any sense essential to this strategy that there be no propositional accompaniment or reaction to it. Shepard simply argues that problem solving of the type undertaken by his subjects could not go forward and achieve the behavioural results that it does except through the operation of a process of image rotation. If Dennett wants to argue that there is a kind of propositional monitoring

of this process going on--that we 'focus in' on certain stages of the image rotation (presumably those defined by the questions the subject puts to himself) rather than just taking note of the completed rotation, then I think Shepard would have no objection. There is still a rotation for all intents and purposes.

Now Dennett, I have no doubt, would be quite unwilling to accept this interpretation of his position. In fact he would probably want to insist that it has been his intention all along to argue the more stringent thesis--specifically, that there is no image rotation, there is instead, a series of discrete propositional episodes which somehow 'name' or describe stages of a rotation (either of images or of the actual objects) if it were to take place. But if this is what Dennett has in mind, then I think that he is espousing a highly dubious empirical thesis. It would be difficult and, I expect, virtually impossible for anyone to carry out a spatial calculation of the required type exclusively within the confines of a propositional format.

Consider the mechanics of such a calculation. According to Dennett, the subject proceeds by asking himself questions about the object pair--questions having to do with how the objects 'would look' relative to one another if one object in the pair were tilted just a few degrees toward the other. In the absence of any kind of imagistic guide, the subject must answer this question in the abstract, by making certain inferences. But what are these inferences? Dennett is not explicit on this point. Presumably they would have a form something like this: If object Y is rotated a few degrees (say 10°) in a clockwise direction, all the parts of Y will be positioned (say) $1/8$ " further

to the south-east of their present position, and this means that Y will look more like X in this new orientation than it did before. The subject then proceeds to consider a second increment of orientation for Y and if there is reason to infer that the objects (X and Y) will look even more alike than they did in the original increment, the subject continues to consider a further increment, and so on until he is convinced that X and Y could be rotated into a position of congruence or superimposition.

It will be noted that never once during this entire process does the subject, according to Dennett, have access to a model or representation of Y in any of its new orientations. The basis of the subject's 'inference to similarity' for each of the increments of Y, is the proposition that Y is $1/8''$, or $1/4''$, or $3/8''$ (and so on as the case may be) south-east of its original position. Now my question is this: Does this propositional description give the subject the kind of information which would enable him to infer a specific fact about the visual appearance of Y--that it 'looks' (to use Dennett's own words)--more like X than it did before? I hardly think so. For how can the subject know what Y is going to 'look like' relative to X, if he does not know what Y 'looks like' in itself (i.e., in its new orientation)? After all, the subject has never seen Y in this orientation and he is barred from doing anything about this by Dennett's abolition of imagery from his representational repertoire. The point is that the propositional description of Y in each of its new orientations is only useful to the subject in his calculations of visual appearance to the extent that the subject is able to generate some kind of visual impression of Y under

the guidance of this description. The simple stipulation that Y is in such-and-such a position does not give any clear indication of its visual appearance, let alone of its visual appearance relative to a second object.

Furthermore, Dennett's account presupposes that there will be a consistent increase in the appearance of similarity between X and Y at each stage in the rotation of Y. But this, surely, is an unwarranted assumption. Is it not likely that the situation in respect of some object pairs at least will be quite different from this--that it will take a whole succession of stages in the rotation of Y before there is even the slightest appearance of similarity? Or perhaps even more dramatically, might it not be the case that for the first few increments in the rotation of Y, Y will appear to be even less and less like X than it did in its original orientation, and it only begins to appear similar to X at a much later stage of the rotation process, say, past the 60° mark? Surely, from a strictly logical point of view there is no reason to assume that just because one thing is becoming more like something else, it must give the appearance of doing so at every stage along the way.

Consider a particular case of objects in rotation. The objects which follow (call them 'X' and 'Y'), are the same three-dimensional shape. Taking X as our standard or model, in the first frame Y is 220° out of congruence with X. Suppose that Y is rotated 10° in a counter-clockwise direction toward X. Does it look any more like X than it did in its original orientation? No. Suppose Y is rotated a further 20°. (This presumably would be stage two of the rotation

according to Dennett's account.) Does Y look any more like X? Once again, I think not. In fact, it is not until Y has been rotated a full 170° from its original position so that it is only 50° out of congruence with X that it begins to look like X.

Figure 9



Y is 220° out of congruence with X



Y is rotated 10° in a counter-clockwise direction



Y is rotated a further 20°



Y is 50° out of congruence with X

In itself this fact about the degree of similarity displayed by object pairs undergoing a series of rotations is neither surprising nor particularly significant. But against Dennett's overall strategy for problem solving, it is decisive. For according to this strategy, the subject is justified in proceeding to a consideration of a further stage in the rotation of Y only if there has been an increase in the appearance of similarity of Y relative to X. But, as I have argued above, in the case of some object pairs, the increase does not occur systematically--for the first few increments in the rotation of Y, there is no appreciable increase in the degree of shape similarity displayed by the objects. This being so, the subject, following Dennett's strategy, would be compelled to conclude prematurely that X and Y are not the same shape. In other words, Dennett's strategy of calculation by 'propositional rotation' would, in this circumstance, systematically militate against the achievement of the solution of the problem.

I have argued that there are two possible interpretations of Dennett's account of the spatial calculation involved in Shepard's experiments. On each of these interpretations, that account is found to be unsatisfactory. The account presented in the first interpretation does not present a genuine alternative to the image hypothesis. There is no denial of the operation of image phenomena--there is simply the suggestion that there might be some mental-propositional accompaniment to the rotation process occurring in the brain. We talk to ourselves or make inquiries about certain stages of that process. The account presented in the second interpretation is a genuine alternative in that propositional episodes supposedly bear the weight of the

cognition. I argue, however, that this account has practical liabilities. The subject could not even solve the Shepard-type problems, let alone exhibit the appropriate reaction-time effect, if he is constrained to carry out the calculation in accordance with Dennett's strategy of rotation by propositional increments.

4.5 The Tacit Knowledge Strategy

The fourth and final strategy for spatial problem solving in a linguistic medium to be found in the literature has been espoused by psychologist Zenon Pylyshyn.¹ Pylyshyn argues that spatial cognition is 'cognitively penetrable'--that is, the knowledge, beliefs, and goals of the subject or subjects involved in the problem solving exercise weigh significantly in the course and outcome of the overall processing.

¹Pylyshyn's is by far the most extensive treatment and critique of the image hypothesis in the literature. In addition to the work which he has published denouncing the hypothesis in general, three of Pylyshyn's articles give special treatment to Shepard's experiments. ("What the Mind's Eye Tells the Mind's Brain: A Critique of Mental Imagery," Psychological Bulletin, vol. 80, pp. 1-24; "The Imagery Debate: Analog Media versus Tacit Knowledge," Imagery, Ned Block, ed., 1981, op. cit., and "The Rate of Mental Rotation of Images: A Test of a Holistic Analog Hypothesis," Memory and Cognition, vol. 7, 1979, pp. 19-28). Pylyshyn's ideas have been discussed in some detail by Kosslyn and Pomerantz ("Imagery, Propositions and the Form of Internal Representations," Cognitive Psychology, vol. 9, pp. 52-76) by Kosslyn, Pinker, Smith and Schwartz ("On the Demystification of Mental Imagery," The Behavioral and Brain Sciences, vol. 2, 1979, pp. 535-581); by John R. Anderson ("Arguments Concerning Representations for Mental Imagery," Psychological Review, vol. 85, pp. 247-277); and by John T.E. Richardson (Mental Imagery and Human Memory, New York: St. Martin's Press, 1980). Roger Shepard, however has not undertaken a defense of his position relative to the arguments articulated by Pylyshyn. Shepard appears to be more concerned with conducting his experiments and discussing the results than with a systematic defense of his overall position relative to particular critics.

For this reason, says Pylyshyn, it cannot be the case that spatial problem solving of the type that Shepard investigates, goes forward on a non-linguistic basis, in a non-linguistic medium or format. In order to substantiate this claim about cognitive penetrability. Pylyshyn asks us to undertake the following mental exercise: Imagine holding in your two hands and then simultaneously dropping two objects--a rock, say, and a maple leaf. "Which object," Pylyshyn asks us to consider, "in your image hits the ground first?"¹ Imagine, then, turning a large heavy flywheel by hand. Now imagine applying the same torque to a small aluminum pulley. Again he asks, "Which one completes one revolution in your image first?" For most people, these imaginings unfold naturally and effortlessly, without any apparent need to reason through what would happen. In their respective imaginings, the rock hits the ground before the leaf and the pulley completes one revolution first.

But, Pylyshyn would insist, it is a mistake to assume from the automatic way in which the imagery unfolds and from the regularity of the content of that imagery (across subjects) that the underlying cognitive process is somehow an autonomous biological process which operates exclusively under the constraints of the neural tissue in which it is instantiated. On the contrary, such results could and ought (in Pylyshyn's opinion) to be explained on other grounds. People have the imagery experiences they do because of the nature of their experience of the world. Heavy objects like rocks have been observed to fall more

¹Zenon Pylyshyn, "The Imagery Debate," in Imagery, N. Block, (Ed.), op. cit., p. 168.

quickly than light objects like leaves or feathers.¹ Pylyshyn writes:

Whatever happens as the sequence unfolds under one's mind's eye is a function of what principles one believes govern the events in question. Clearly, the laws of dynamics or optics and the principles of geometry that determine the relation, say, between the perimeter and the area of a figure are not intrinsic (built in) to the representational media or to the functional mechanisms of the mind. Not only must one have tacit knowledge of them, but the way in which imaginal events unfold naturally can usually be influenced with considerable freedom simply by informing the subject of the appropriate principle. Thus what seems to be a natural and automatic unfolding process is cognitively penetrable--that is, it is under the control of an intellectual process with all that this implies concerning the intervention of inferences and reasoning through.²

¹In truth, of course, it is not the greater weight of the rock which accounts for its speedy descent to the ground--it is its size and shape. Conversely, it is the particular size and shape of the leaf which accounts for its relatively slow descent. The rate of acceleration of bodies falling in space is, strictly speaking, a function of the force of gravity. What affects this rate are factors such as size, shape, and wind velocity. Two identically-shaped objects--say two cannon balls--a conventional cannon ball and a hollow one would, theoretically, reach the ground at the same time. But these facts of physics aside, the point which, I take it, Pylyshyn is concerned to make here is that people develop their expectations about the behaviour of physical objects on the basis of their observations. And, in the ordinary course of things, we do not have the opportunity to observe object pairs such as hollow cannon balls and conventional ones falling simultaneously. (And even if we did, we would probably have to have the principle of physics represented by such a pair pointed out to us.) We see, rather, rocks falling and leaves falling (separately) and are struck by the gentle drifting of the leaf by comparison to the direct descent of the rock. And so we come to infer (incorrectly, as it happens in this case), the principle that rate of acceleration of falling bodies increases with weight. Pylyshyn is suggesting here that these inductively induced expectations govern the imagery that we have so much so that if we were asked to imagine the two cannon balls falling we would probably 'see' the heavy one reach the ground first.

²Ibid., p. 169.

Thus for Pylyshyn, if people's experience and understanding of the physical world were substantially different, if for instance in the case described above, it did not seem to them that the rate of acceleration of bodies falling in space increases in proportion to weight, their imaginings relative to such circumstances would be appropriately altered. (They might, that is, imagine the cannonballs hitting the ground simultaneously.) Accordingly, Pylyshyn proposes a simple behavioural test which can be used to detect the presence of the condition of cognitive penetrability. If a certain behaviour pattern can be shown to be systematically connected to certain beliefs which people (for good or bad) are known to hold then, says Pylyshyn, we must conclude that the explanation of that behavioural pattern must appeal to operations upon symbolic representations such as beliefs and goals--the explanation must, in other words, contain rule-governed cognitive or computational processes.

Suppose, says Pylyshyn, that a group of Shepard's subjects are presented with geometrical figures which have the appearance of being unusually heavy. And suppose that it is their belief that all things being equal, heavy objects take longer to rotate about an axis than light ones. On the tacit knowledge account, we might well expect that this belief would be reflected in the subjects' reaction-time behaviour--that it would take the subjects longer to solve a problem involving objects at a 60° orientation disparity in a case where the objects in question are (believed to be) heavy than in a case where the objects in question are (believed to be) light. And what if in fact this expectation is borne out experimentally? How do we explain

this apparent correlation between the pattern of the subjects' behaviour and their beliefs? Do we say that the correlation is not explicable in logical terms and that the real explanation has to do with the nature and operation of analogue mechanisms in the brain? Or do we attempt to find an alternative 'inferential' explanation--one which makes explicit reference to the underlying physical beliefs of the subjects? Pylyshyn argues that it is the latter explanation which is required here. He writes:

... it is clear that in this case the explanation must appeal to the subjects' knowledge of the behaviour of heavier objects rather than to any intrinsic property of images. It is our contention that such an appeal to tacit knowledge will be required ... this is the primary reason for preferring a propositional to an analogue account of mental processes.¹

Now in point of fact, Pylyshyn does not actually test for the reaction time/weight correlation postulated above. He appears to offer this rather, in an effort to concretize for us the general character of the behavioural test which might be applied in order to detect cognitive penetrability. What he does is make a more general claim. This is the claim that 'tacit knowledge' can explain (and by implication is causally responsible for) the so-called mental rotation phenomenon. But what kind of 'tacit knowledge' does Pylyshyn have in mind here? Minimally, I take it, it must fulfill the following requirement: it must be knowledge the having of which will cause the subjects to behave

¹Z. Pylyshyn, 1979, op. cit., p. 20.

in accordance with the reaction-time pattern described by Shepard.

It must be conceded that Pylyshyn's idea that people in general have developed a kind of rough-and-ready physics based on their observation of the physical world has some merit. Our experience of the physical world and of the dynamics of its operation is pervasive--we observe spatial problems being worked out in physical terms all the time. It would not be unreasonable, then, to suppose that, with repetition, these observations could assume for us the status of physical laws and that these so-called laws could infect our thinking in subtle yet fundamental ways. They might, in effect, constitute a kind of informal information base for our inferences about objects and their interaction. And it could well be that such information is only available to us at an intuitive or unconscious level. We have access to this body of information, that is to say, only when we are acting upon the world (eg. playing baseball) or perhaps when we are engaged in what we call visualizing some physical process, but not when we have to reason verbally or to answer certain kinds of questions in the abstract.

It must also be conceded in Pylyshyn's favour, that people in general have inferred certain rather general principles about the dynamics of objects in rotation. In particular, they can be expected to have inferred that there is a correlation between 'rotation time' and 'distance of rotation'--that all things being equal, it takes longer to complete a considerable rotation than a minimal one. No doubt, the observations relevant to such an inference would have been made at a very early age under relatively mundane circumstances. Children playing with construction blocks, working a jigsaw puzzle, or learning for the

first time to fit a key in the front door of their houses will have had to perform rotation-like manipulations and will have noticed that in some situations--when, for instance, the puzzle piece has to be turned all the way around--that it takes longer to complete the immediate task of filling in the space in the puzzle than in other circumstances--when the puzzle piece has to be twisted just a small degree.

So let us suppose, with Pylyshyn, that people in general and Shepard's subjects in particular, do indeed have some such tacit knowledge available to them. They harbour, that is to say, a strong (though inarticulate) conviction about the length of time a given rotation ought to take--specifically that a rotation over many degrees will take longer than a rotation over few degrees. Let us suppose as well, (and this time strictly for the sake of argument) that such a conviction or belief could, at an unconscious level, influence the course and outcome of the ultimate problem solving activity in the sense that it provides a sort of background assumption (possibly among others) for the subjects' reasoning. Can we, on this basis, go the further step and say that having this belief the subjects are bound to (or even likely to) exhibit the reaction-time pattern described by Shepard?

I do not think so. First of all, the reaction-time pattern in question is very precise. Shepard's subjects exhibit reaction times which are linearly related to the degree of orientation disparity of the test stimuli. With every increase in orientation disparity there is a proportionate increase in the length of time the subjects take to reach a decision. On the other hand, the kind of tacit belief which we have granted people in general are likely to have about rotating objects

and which, according to Pylyshyn's ostensible theory, is supposed to produce the reaction time in question, is very rough and ready indeed. At its most articulate, it expresses an expectation that there ought to be a temporal/spatial correlation--a correlation, that is, between rotation time and degree of rotation, but nothing more explicit than this. In particular, precise linearity is not part of this belief.

Now we might well discover that upon explicit questioning some people will be prepared to elaborate on their tacit belief regarding the rotation time/rotation distance correlation by suggesting that the increase of the former relative to the latter could be monotonic and so perhaps come eventually to espouse a belief in linear gradation. It seems to me, however, that this cannot properly be considered a constituent of the so-called system of tacit physical belief which Pylyshyn has postulated. It represents, rather, an after-the-fact working-out of a logical entailment inspired, it will be noted, not by experience but by our (Socratic-like) questioning.

In fact, I would suggest that the idea of linear gradation is not something which can be generally inferred from experience at all. The reason for this is simply that our ordinary experience of the world is too haphazard to make possible the observation of precise temporal patterns. We notice a pattern only if it is striking in some very immediate practical sense, and then only if it is a pattern detectable without precise measurement. The situation in which a puzzle piece has to be rotated almost in a full circle is noteworthy perhaps because of its difficulty (this is especially true for small children for whom the requisite manual dexterity necessitates considerable concentration) and

the length of time it takes to complete the manipulation is noteworthy as a concomitant of this. But we do not note as a matter of course just how much more time is required in this case than in a case where a piece has to be rotated only a short distance. Nor do we note just what the differences in relative distance of any two rotations is. They are just noted as being 'longer' or 'shorter' relative to one another. This might well be adequate (with the repetition of such episodes) to give the impression that there ought to be some kind of correlation between rotation time and rotation distance, but without precise measurement such as occurs in a controlled experimental environment of the length of time relative to the length of distance, a pattern of precise linearity is not detected.¹

¹That Shepard's reaction-time pattern is not something which can be observed in the course of our ordinary comings and goings with the world is well-attested. Roger Shepard and Lynn Cooper (in Mental Images and Their Transformations, 1982, op. cit., p. 10) and W.K. Estes (in Handbook of Learning and Cognitive Processes, vol. 5, 1978, p. 2) have on separate occasions acknowledged the significance of the development in cognitive psychology of chronometric measurement techniques and instrumentation. Without these, the psychologists claim, the linear correlation between reaction time and rotation distance could never have been detected. The fact that must be appreciated is that on a subjective - impressionistic kind of analysis of timing - by just watching people, for instance, we would never notice a difference of several milliseconds between one reaction time and another. To the naked eye, assisted only by the second hand on a watch, all or most reaction times in a given experimental sequence, would appear to be roughly the same. We need, therefore, sophisticated timing techniques and instrumentation in order to make obvious to us the difference in reaction time and thereby the pattern of reaction time relative to the distance variable. Shepard says this:

On introspection, then, the mind's most efficient and automatic operations appear to the mind itself to be virtually instantaneous as well as effortless. Little wonder that philosophers like Hume, who based their conclusions on the subjective observation of their own mental processes rather than the use of more objective and faster-operating physical

I would argue, then, that even if we suppose Pylyshyn's basic thesis regarding the existence of tacit knowledge and the likelihood of its significant influence on cognitive processing, we do not have the specific kind of tacit knowledge available to us which is relevant to the production of the reaction-time behaviour which is in question here. To put a fine point on this, we do not have a belief in the linear gradation of rotation time relative to rotation distance and so cannot in any sense be construed to be governed or guided cognitively and, ultimately, behaviourally, by such a belief.

This brings me to a second, more fundamental criticism of Pylyshyn's position. There would seem to be absolutely no reason why, given his position, a belief about the temporal character of a rotation process should affect the ostensible cognition involved in the solution of Shepard-type problems at all. After all, this cognition has to do with very specific problems--it has to do with the similarity or dissimilarity of shape of two geometrical figures. Pylyshyn has been at some pains to maintain, rotation, mental or otherwise, has no significant role to play in the ultimate computation of this. People may well be under the introspective impression that they solve spatial problems by manipulating images before their mind's eyes, but, Pylyshyn says,

instruments to clock such processes in others, could speak as if a more complex or extensive mental transformation "costs the imagination no more trouble" (David Hume, An Equity Concerning Human Understanding, 1948, vol. 2, p. 16) than a simpler or less extensive one. It was not until 1969, following the successful application of refined chronometric techniques to processes of mental comparison ... that we actually began our attempt to time mental processes. (R. Shepard and L. Cooper, Images and Their Transformation, op. cit., p. 10.)

this is a false impression. They only think that they solve spatial problems in this way because that is how they have always seen such problems solved in the physical world. On the contrary, according to Pylyshyn, the essential cognitive process involved in spatial cognition generally and involved in Shepard-type problem solving in particular, is inferential. Now if this is so, if Shepard's subjects are not rotating images but making inferences to shape similarity, then it seems to me rather clear that information having to do with rotation time or reaction time is irrelevant. How long it ought or ought not to take to complete a rotation has nothing to do with the shape similarity of two objects. I would argue, then, that even if we were to grant that people might have a tacit belief in the linear gradation (of rotation time relative to rotation distance) there is no reason for that belief to enter into the cognition--that cognition being, so Pylyshyn says, an inference to similarity.

Thus Pylyshyn's tacit knowledge account of Shepard-type problem solving does not provide a viable alternative to the analogue hypothesis. Its major liability is that it, like the other alternative strategies I have considered, fails to explain the reaction-time behaviour of Shepard's subjects. In the first place, we do not have the kind of tacit knowledge relevant to the production of this reaction time (we have no opportunity to develop such knowledge outside of an experimental context) and in the second place, there is no reason for our tacit knowledge of the physics of rotating objects to influence our (inferential) computation of shape similarity.

It will be noted that in addition to his particular treatment of the mental rotation phenomenon, Pylyshyn has been concerned to promote a second, 'underlying' or 'generic' thesis regarding the operation of the so-called faculty of imagination. It is Pylyshyn's notion that this faculty cannot generally be considered to operate autonomously--it is subject to the influence and, indeed, the governance of the tacit beliefs held by the person(s) in the process of imagining. In respect of this generic thesis, I am inclined to concede that there would well be some truth buried here. Perhaps in some circumstances, there is a logically coherent relation between the imagery experiences that people report and the tacit beliefs that they have. Perhaps it is the case, for instance, that trained physicists would (if required to perform the mental exercise described earlier) report 'seeing' before their mind's eyes the two objects (of comparable shape and size but of different weights) reach the ground simultaneously, while the ordinary population, drawing their tacit physical beliefs on an ordinary experience basis, would report seeing the heavy object in the pair hit the ground first. (I do not know. This obviously requires empirical investigation and it would seem to be incumbent on Pylyshyn to provide it.) And, if some such correlation is established then it is perhaps arguable that an inferential or 'tacit knowledge' explanation of the cognition in this circumstance, is required.

Having said this, however, I do not think that the image hypothesis or image explanation (for the cognition of Shepard's subjects) is jeopardized in the least. This is one instance (quite possibly among many?) where the faculty of imagination appears to be operating autonomously--in accordance with principles which are not acquired but 'pre-

wired', so to speak, in the human brain on a species-wide basis. It may be the case that in some instances of the operation of what we commonly describe as imagination a tacit knowledge explanation is required but it is just not the case in respect of the cognitions of Shepard's subjects. There is no explanation in terms of tacit knowledge for the behaviour of these subjects.¹

4.6 Considerations of Efficiency

In the final analysis, then, the hypothesis of image rotation seems to furnish the only currently available account of the principal finding of Shepard's experiments: namely, that the subjects' reaction times increase linearly with the degree of orientation disparity exhibited by the test objects. Four alternate linguistic strategies for spatial problem solving have been postulated and discussed. Out of this group,

¹For the record, it should perhaps be mentioned that Pylyshyn has conducted a brief course of experiments (reported in Pylyshyn, 1979, op. cit., pp. 19-28) designed to prove the knowledge dependent character of mental rotation. Pylyshyn claims to have shown in these experiments that certain (cognitive) factors such as the inherent structural complexity of the test objects and the degree of practice which the subjects have attained, increase and decrease, respectively, the subject's reaction times. I have chosen not to deal with these experiments because they are considered to be controversial within the discipline--Lynn Cooper and Peter Podgorny have conducted a similar course of experiments (reported in "Mental Transformations and Visual Comparison Processes: Effects of Complexity and Similarity," Journal of Experimental Psychology, vol. 2, 1976, pp. 503-514) the results of which disconfirm (or at any rate throw into question) Pylyshyn's results. Also Steven Kosslyn and his associates (Pinker, Smith, Shwartz) have argued very convincingly (1979, op. cit., pp. 535-581) that Pylyshyn's data (if accurate) can be explained by inherent properties of the neural tissue--in the case of practice, for instance, by the gradual strengthening of synaptic connections with repeated firing. Accordingly, I have chosen to restrict the discussion to what is certainly the philosophical heart of Pylyshyn's position (the explanation by 'tacit knowledge' thesis). I shall leave the presentation and interpretation of data to the psychologists.

two strategies, specifically, Zenon Pylyshyn's 'tacit knowledge' strategy and Daniel Dennett's 'rotation by propositional increments' strategy, have been found to be deficient because they stipulate problem solving techniques which are actually incapable of resolving the similarity/dissimilarity question. The other two strategies, the 'wholistic description' strategy postulated by Anderson and Flanagan and the 'feature-by-feature description' strategy postulated by Just and Carpenter, stipulate problem solving techniques which seem in principle to be capable of resolving the similarity/dissimilarity question (though with considerable difficulty), but which could not reasonably be expected to give rise to the specific kind of behaviour pattern exhibited by Shepard's subjects.

Having said this, however, I cannot rule out the possibility that someone will yet devise a strategy for problem solving which satisfactorily accounts for Shepard's reaction-time finding without making reference to anything like mental images and image rotations. What, then, if some such strategy is devised? Does this mean that the strategy of image rotation ought automatically to be set aside? I think not. That it may be possible to contrive a linguistic strategy for the kind of problem solving involved in Shepard's experiments, does not mean that such a strategy is necessarily the correct one. It simply means that the calculation at issue here could be made in linguistic terms. But the question of whether in fact it is so made, is still to be answered.

Now suppose, for the sake of argument, that this situation does indeed arise and that we have two possible strategies for spatial problem solving which account for the Shepard reaction-time effect--the

image rotation strategy and a (yet to be devised) linguistic strategy. Are we obliged then to simply acknowledge a situation of stalemate? Or are there grounds which can be adduced which tip the balance in favour of one or the other strategy?

It seems a reasonable assumption that the human brain processes information in a way that maximizes the efficiency and optimality of its performance. This means that a constraint on any theory of cognitive representation is that it not propose that the brain is processing information inefficiently. Unfortunately, it is not always a trivial matter to decide how the efficiency of a given system or organ should be measured in the absence of detailed knowledge of its physiological implementation. J.R. Anderson¹ has proposed that the efficiency of a system can be measured in terms of the efficiency of its computer simulation. If some such proposal were accepted, it would be possible to place considerable constraint on a theory. However, it is unlikely that there is going to be widespread consensus in the field about such a definition. Nonetheless, it seems possible to apply very general (i.e., non-implementation specific) notions of efficiency to impose some constraints on theories of cognitive representation.

One very general consideration leads to an interesting conclusion. It is the case that well-designed systems tend to have special representations for the kinds of information they have to process frequently. These representations are designed to facilitate the kind of computations useful for this kind of information. For instance, we know from

¹John R. Anderson, 1978, op. cit.

physiological evidence, that visual and auditory information is given very different encodings at initial neural levels. Another example of this comes from an advanced computer language like Interlisp.¹ Interlisp has list structures that are useful for encoding symbolic structures (like propositions), arrays that are useful for encoding dimensionally organized information (like pictures) and even some string capabilities useful for encoding sequential information (like verbal input). The array and string capabilities are additions to the original Lisp² language which had only list structures. These additions were forced by the practical needs of doing various types of information processing operations in Lisp.

Now assuming that the brain is a 'well-designed system' and given that spatial decision making is a day-to-day, if not moment-by moment concern (we have to make such decisions every time we move about the physical environment or make use of a tool), it seems reasonable to suppose the evolution of a representational system the basic structures of which are as analogous as possible to the spatial structures in the world which they represent. Equally, it seems reasonable to suppose that such a system would have available to it, analogue processes - that human beings might do in their mind/brains what they would do in the physical world should it be possible, safe, and convenient.

¹W. Teitleman, Interlisp Reference Manual, (Palo Alto, California: Xerox Palo Alto Research Center, 1976).

²J. McCarthy, P.W. Abrahams, D.T. Edwards, I.P. Hart, and M.J. Levin, Lisp 1.5 Programmers Manual (Cambridge, Mass.: MIT Press, 1962).

Rotation is one such process. After all, as any child will tell you, the easiest and most definitive way to determine whether or not two objects are the same shape is to try to match them together physically. If one is barred from doing this by practical considerations, then the next best thing is to try to achieve a match in theory.

There is a further point about efficiency which bears consideration. In the discussion of the wholistic description strategy, attention was drawn to the difficulties inherent in providing an adequate linguistic description for Shepard's test objects. This is a problem not just for the wholistic description strategy, but for descriptive strategies per se - to the extent that the spatial properties of the test objects must be transcribed into a linguistic format, an adequate spatial vocabulary must be provided as well. This difficulty is side-stepped, however, in fact the transcription step is eliminated altogether, if we suppose along with Shepard, that the spatial properties of the test objects are not encoded in the brain linguistically, but that they are encoded imagistically - in structures which are internal analogues of the corresponding physical realities. On this supposition, the need to provide a transcription of the spatial information simply disappears.

4.7 Conclusion

I have been arguing the merits of the case for the mental rotation strategy for spatial problem solving relative to the outstanding linguistic opposition. Roughly, the merits seem to be twofold: First of all and most particularly, mental rotation is the only strategy which

has thus far been adduced which can actually be seen to bring about the reaction-time behaviour which Shepard's subjects exhibit. Secondly, this strategy and the analogue system of cognitive representation which is consistent with it, are favoured by considerations of efficiency. An image or analogue system of cognitive representation is capable of processing spatial information in its own terms. In the particular case of Shepard-type problems, this means that the global shapes of the test objects can be processed as global shapes. The shape information does not have to be transcribed into an alternative language prior to the actual similarity/dissimilarity assessment. On the contrary, that assessment can be taking place even as the rotation proceeds.

CHAPTER 5

The Case In Principle Against Imaging

5.1 Introduction

Now it seems to me that philosophers and psychologists have advocated the non-image rotation stance relative to Shepard's imagery research at a fairly considerable theoretical cost. For aside from the fact (which I sought to establish in Chapter 4) that the various linguistic strategies turn out, on analysis, to be incapable of accounting for the reaction-time phenomenon, they rely on a base of supposition, which is, to put it plainly, too obviously contrived to be believable. Indeed, I venture to say that contrivance is the single most striking feature of the theories as a group.

Clearly, it is essential to the linguistic strategies (if they are to function in their appointed role as the official opposition) that a link be established between linguistic/propositional problem solving per se and the orientation factor. Somehow the linguistic theorists must show that the relative spatial positioning of the test objects affects the course and outcome of a linguistic cognition. Since linguistic structures and inferential processes are not normally or naturally constrained by spatial considerations, the linguistic theorists have had to arrange for this. They have had to address themselves specifically and deliberately to the question, 'What do we need to suppose to be the case if the reaction-time behaviour pattern exhibited by Shepard's subjects, is to come about by linguistic means?' And they have been forced

into the difficult and, I dare say, undesirable position of having to make rather extravagant suppositions.

In the case of the feature-by-feature description strategy, for instance, the required link is provided on the supposition of an unusual kind of descriptive tactic. Instead of describing each object separately and wholistically, the subjects are supposed to go about generating these descriptions on a piecemeal basis. They identify and describe a single feature on one object (the 'standard') and then attempt to locate this feature on the second object and so on for all the features present.

The tacit knowledge strategy relies on a pair of suppositions: the supposition that people in general, and Shepard's subjects in particular, have acquired a belief about the length of time it ought to take to find the shape similarity of two objects by rotation and the supposition that this belief enters into the subjects' inference to similarity in an unconscious but significant way - causing the subjects to take longer to complete the inference in the case of an object pair displaying a considerable orientation disparity, than in a lesser disparity situation.

Daniel Dennett's theory of rotation by propositional increments operates on the supposition that the subjects undertake to 'mimic' a physical rotation propositionally. They ask themselves a series of questions having to do with the visual appearance of one object relative to the other, should it be turned 'just so far'. And the questions occur in a sequence appropriate to the course of a rotation of the test objects if it were actually to take place. This apparently

means that the greater the orientation disparity displayed by the test objects, the greater number of questions privately asked.

We can, of course, suppose with Dennett that people perform rotations non-spatially. But really why should we suppose this? More to the point, is there any justification for this supposition other than that Dennett apparently thinks it is required if his propositional theory is to be retained in the face of Shepard's data? We do not find, for instance, that people report propositional rotations. Nor has it been demonstrated to be the case that people can be trained to perform such rotations on paper - writing out successive descriptions of the appearance of one object relative to the other if it were rotated in stages about an axis. Nor is there any reason to believe that such a strategy would be an efficient way to solve the spatial comparison problem. Digital computers do not employ the propositional rotation strategy. And it is not even clear that they could be programmed to do this.

Equally, we can suppose with Pylyshyn that people's inferences about shape similarity are influenced by their beliefs (granting for the moment that they have such beliefs) about rotation. But again we must wonder why anyone would want to suppose such a thing. There is no connection - logically speaking - (except in the theory being challenged) between information relating to the length of time it takes to complete a rotation and the issue of the shape similarity of two objects. So to suppose, as Pylyshyn apparently does, that the rotation belief enters into the calculation process in a cognitively significant way, is to suppose that that process is essentially and consistently

invalid. And surely we have every reason to believe that it is not invalid. In the vast majority of cases, people (or at any rate a representative sample of people) come up with the correct assessment of the objects. And it is hardly likely that this kind of consistency would result from invalid reasoning.

And we can suppose that Shepard's subjects are employing the feature-by-feature description tactic relative to the comparison problem. After all, as I argued in Chapter 4, there is no reason in principle why it would not be possible to solve the problem (though not to mimic the reaction-time effect) on a feature-by-feature basis. But even here we are bound to consider this an unlikely possibility - surely no reasonable person would pursue this tactic when the simple description of each object in turn, will accomplish the comparison with less difficulty and less room for error. Indeed, the only basis on which the supposition makes any sense at all is if we are prepared to suppose as well that the subjects themselves have, for some reason, a desire to mimic the Shepard reaction-time effect, and have come to settle upon the feature-by-feature tactic (unnecessarily as it happens) as the means of securing this end.

I suggest, then, that the various suppositions which lie at the heart of the linguistic strategies are gratuitous, so gratuitous that one is compelled to wonder to what purpose they have been supposed by the linguistic theorists in the first place. Why would an otherwise careful philosopher like Daniel Dennett, for instance, willingly place himself in the awkward position of having to propose and defend something like propositional rotation? Surely he would be better advised

to simply adopt image rotation as the (or, at any rate, a promising) explanation of the reaction-time phenomenon?

This may be the reasonable course but Dennett and the other linguistic theorists are apparently determined - do-or-die - to avoid it. In the face of such determination one is compelled to draw the conclusion that the analyses of these theorists, in respect of Shepard's work, have all along been guided by ulterior motives. For some reason they have taken it to be necessary to deny the legitimacy of cognitive explanations couched in terms of images and image rotations, indeed, they seem to see themselves as having a duty to do so.

What, then, is the explanation for this out-of-hand rejection of image rotation? Why do Dennett, Pylyshyn and others take it to be a foregone conclusion that however, ultimately, the reaction-time phenomenon comes to be explained, the image/analogue explanation is simply unacceptable - indeed impossible? Why do they begrudge the postulation of images so?

5.2 Thinking in Images: The Conventional Theory

There is a simple answer to this question. Apparently, the linguistic theorists believe that there is more at stake here than the simple acceptance or rejection of an explanation for a specific kind of spatial cognition. Apparently, they believe that acceptance of the image rotation theory brings with it a whole 'Pandora's Box' of undesirable theoretical consequences. Wittgenstein and Ryle in philosophy, and John Watson in psychology nailed the lid on this box some years ago and so the present generation of philosophers and psychologists are

reluctant to re-open it. I suggest that the Pandora's Box has to do with the problem of meaning - what the linguistic theorists fear most, and rightly so, is that they will be committed to the view that images are (sometimes) the vehicles of thought in the strong sense that thinking is identifiable with imaging.

Undoubtedly, there is some basis for this fear. In the history of philosophy and psychology, support for imagery has almost always gone hand-in-hand with some version of the imagist theory of meaning. The classic example of this is to be found in the work of the British Empiricists. Locke and Hume maintained that having an idea or concept is really having an image experience of a particular sort. We receive impressions of heat or cold, thirst or hunger, pleasure or pain, and the mind makes images or copies of these impressions, which are called ideas. These ideas give rise in the mind to feelings of desire or hope or fear. The mind in turn makes copies of these new ideas and thus there comes into being another set of ideas.

Locke and Hume believed imagery to be ideally suited to the task of explanation because images seem to require no interpretation at all. It is this feature that images have been taken (not just by Locke and Hume but by empiricists generally, both classic and modern) to share with pictures, and, in turn, it is one of the features that is supposed to set pictorial modes of representation apart from linguistic modes. Images are not, so it is supposed, arbitrarily or conventionally related to the things they represent such that their particular 'representational functions' would have to be learned, they simply resemble these things. The meaning of the image is dictated by its very nature

- an image of a coffee cup does not appear like or indeed stand for a kitchen sink. Psychologist George Humphrey describes and endorses this particular understanding of the logic of image representation:

... when we have an image of a boy dressed in blue, what is happening is that we are imagining the boy directly. We do not first make an image and then bring in some additional process which tells us its meaning, i.e., that this is an image of a boy. If we are asked what colour the boy's coat is, we can say that it is blue just as though we were actually perceiving the boy and were asked the same question. Thus the meaning of the image presents no problem.¹

What causes difficulty, Humphrey goes on to say, is the insistence, on the part of some people, that imagining and interpreting the 'imagined product' must be distinct cognitive operations. There is no reason to assume this, says Humphrey, images wear their interpretations 'on their sleeve', so to speak. We know what an image is an image of - simply as a concomitant of its very construction - because it 'looks like' what it represents.

Likewise, Hume endorses the representation-by-resemblance view. Hume writes,

When I shut my eyes and think of my chamber, the ideas I form are exact representations of the impressions I felt; nor is there any circumstance of the one, which is not to be found in the other. In running over my other perceptions, I find still the same resemblance

¹George Humphrey, Thinking: An Introduction to Its Experimental Psychology (New York: John Wiley and Sons Inc., 1963), p. 225.

and representation. Ideas and impressions appear always to correspond to each other.¹

E.B. Titchener undertakes a more detailed account of how images represent particular objects via some form of resemblance. He cites the following as exemplary cases, culled, so he claims, from his own image repertoire. Notice that Titchener's images are unlike Hume's in that they are not exact copies of what they represent. As Titchener describes them, they are more like impressionist paintings. The key, however, is that for Titchener - like Hume - resemblance counts for representation. An image of a triangle must look like a real triangle. Otherwise, says Titchener, it could not bear the meaning of 'triangle' for him. Titchener writes,

My own picture of the triangle, the image that means triangle to me, is usually a fairly definite outline of the triangle figure that stands for the word 'triangle' in the geometries ... horse is to me, a double curve and a rampant posture with a touch of mane about it; cow is a longish rectangle with a certain facial expression, a sort of exaggerated pout. Again, however, these things mean horse and cow, are the psychological vehicles of these logical meanings.²

Thus, on the empiricist view, language stands in a purely external relation to thinking. Words are to be used as marks of ideas (images)

¹David Hume, A Treatise of Human Nature (Oxford, 1951), L.A. Selby-Bigge (Ed.), Book I, Part I, p. 3.

²E.B. Titchener, Lectures on the Experimental Psychology of the Thought Processes (New York: Macmillan, 1909), p. 19.

in the mind of the speaker. They have no natural connection with ideas but are made into signs by the arbitrary choices of men. Since words are the labels of ideas, the ideas must be present before they are labelled. Wrote Locke,

In the beginning of languages, it was necessary to have the idea before one gave it a name.¹

Thus it would be a logical possibility that a human being with a mind well-stocked with ideas should have failed, for some reason, to assign any marks to them. This person would be occupied with thoughts and observations, but devoid of a public language. He would have a private language, composed entirely of imagery. (See Appendix D.)

The received opinion current in philosophy (and perhaps to a lesser extent) in psychology, is that theories of representation of the sort sketched above are, in several crucial respects, flawed. It is believed that such theories sin against certain conceptual standards - laws even - established in the first instance by Wittgenstein and Ryle and elaborated by a 'second wave' of philosophical critics - V.O. Quine, Norman Malcolm, and Wilfrid Sellars (to name a few). These are standards about the nature of representation, about the possibility of a 'private language' and about what might or might not constitute 'rock bottom' cognitive explanation. Together they comprise what might be loosely described as the 'logical behaviourist' case against the use of

¹ John Locke, Essay Concerning Human Understanding. J.W. Yolton, Ed. (New York: 1961) Bk. III, Ch. 9, Sec. 2.

imagery in cognitive explanations. Any theory, such as Roger Shepard's, which proposes imagery in this kind of explanatory capacity must ultimately be judged against these standards.

The crucial question, of course, remains to be asked: have any of these standards (laws)¹ actually been infringed by Shepard's theory? Would Wittgenstein or Ryle, for instance, if faced with the details of Shepard's reaction-time experiments, have to rule out of court the possibility of explanation in terms of analogue structures and processes in the brain? Would they have to maintain that it is impossible in principle for such structures and processes to exist/operate somewhere/somewhat in the neurophysiological substrate? Would they have to maintain that such structures and processes cannot do the job of representation marked out for them by Shepard? Would they have to maintain that Shepard's theory would entail an infinite regress of explanations?

In what follows, I propose to consider the logical behaviourist case against imagery with a view to deciding whether or not the manifest intolerance of the linguistic theorists in respect of Shepard's theory is justified. I do not propose to enter the miasma of exegetical dispute which surrounds Wittgenstein's private language argument-deciding, for instance, whether or not Wittgenstein should be interpreted as a behaviourist. I intend to treat generic arguments rather

¹I use the expression "laws" here because it seems to me that the Wittgenstein/Ryle reflections on the nature of mind, introspectionism, etcetera, have the status of laws among virtually the entire philosophical community. If there is ever such a thing as conventional wisdom in philosophy, this is it.

than individual philosophers, indicating what I take ^{to be} the gist of these arguments and leaving the questions of scholarship to the experts.

Ultimately, I shall be arguing that Shepard's theory does not run afoul of logical behaviourism - that, indeed, there is no philosophical case as such to be made out against Shepard's theory. Philosophers and psychologists have come to think that there is perhaps because they have interpreted Shepard along conventional empiricist lines. They have interpreted him, that is, as espousing and defending the 'imagist' theory of meaning. They are wrong about this. Shepard is espousing and defending a theory or more precisely, perhaps, a scientific/empirical hypothesis about the physical causal condition(s) which bring(s) about spatial cognition of a particular kind. Images, on this view, are not the bearers of meaning but 'engineering' features of the human brain which are computationally significant. They make a difference to the course and outcome of the spatial cognition. In short, while I do not deny that there is a case - a pretty devastating case - to be made out against theories (like those postulated by E.B. Titchener and John Locke) which postulate images as the basic units of cognitive meaning, I deny that it can be applied in this instance. Shepard's theory requires special consideration and treatment - the 'stock' criticisms of imagery and image theories generally will not do here.

5.3 Four Issues

What, then, are the 'stock' criticisms - the logical behaviourist case, so-called? I take it that the substance of this case has to do with four main issues. For purposes of identification, they may be

called:

- i) the representation-by-resemblance issue;
- ii) the images-as-propositions issue;
- iii) the infinite regress issue; and
- iv) the privacy issue.

5.4 The Representation-by-Resemblance Issue

Consider the representation-by-resemblance issue. Earlier, I indicated that it is a central contention of Locke, Hume and company that images constitute the sum and substance of thought. To think that the university is in flames is ultimately to entertain a mental picture or series of pictures of the university engulfed in fire. This imagery gets its message across - refers to the university, and conveys information about its state - by simply resembling its subject matter. In this, it is supposed, it differs from language which conveys meaning symbolically, on the basis of arbitrary convention. Clearly, such a view assumes that there is no problem in recognizing a picture - say, of a man as a man. Just as anyone who could pick out a real man could identify a mirror image of a man, so, it is thought, could he pick out a pictorial representation.

The logical behaviourists deny this. Indeed they are strongly inclined to doubt the very intelligibility of the suggestion that there is a stage at which cognitive processes are carried out in a medium that is fundamentally non-discursive. The reason for this is simply that, for the logical behaviourists, resemblance is inadequate for representation. Images/pictures must always come under a description

to mean anything.

The logical behaviourist point can be substantiated by way of a consideration of pictures and how they serve to represent. Suppose, for instance, that we are shown a picture of a skyscraper in a large city. Is it obvious from the picture itself what it is a picture of? It could be (could it not?) a picture of a particular landmark skyscraper such as the Empire State Building in New York. Alternatively, it could be an example of a skyscraper for purposes of identifying a kind of building, an exemplar of a class. It could be an illustration for a general article on twentieth century architecture in the West, or even a composition to hang on the wall for its 'artistic quality'. Even if we are shown a realistic picture - a photograph, say, which is readily identifiable as the Empire State Building, it may have a non-pictorial meaning, as in a situation where it and like-shaped displays are used to signify products made in New York City, while Eiffel Tower shapes indicate Paris-made items. Without some accompanying rule - some standard of assessment - it is impossible to tell what the picture is for and hence what its subject really is.

The lesson here is a familiar Wittgensteinian one - meaning is not 'simply given' in pictures. Rather, a picture is a proposition-radical, says Wittgenstein - there are any number of assertions, denials, questions, etcetera which can be made/asked in a picture. Wittgenstein puts the problem of the meaning of pictorial representations this way,

Imagine a picture representing a boxer in a particular stance. Now, this picture can be used to tell someone how he should stand, should hold himself; or how he should not hold himself; or how a particular man

did stand in such-and-such a place; and so on. One might (using the language of chemistry) call this picture a proposition-radical.¹

I see a picture; it represents an old man walking up a steep path leaning on a stick. Might it not have looked just the same if he had been sliding downhill in that position?²

The root of the problem, according to the logical behaviourist, is that pictorial representations are insufficiently abstract to convey meaning on their own. Any picture of a thing will, of necessity, display that thing as having indefinitely many properties; hence pictures correspond (and fail to correspond) in indefinitely many ways to the things they resemble. Suppose we want to say (or think) in pictures or images that the Empire State Building has many windows. A picture of the Empire State Building with many windows might convey this meaning but it can also convey the meaning that the Empire State Building is large or, for that matter, that it is difficult to heat or that it is made out of brick of a particular colour. Notice that a symbolic system of representation such as language is exempt from these worries. This is one of the respects in which language really is abstract. A picture of the Empire State Building with many windows is also a picture of a 'large' Empire State Building. But the sentence "The Empire State Building has many windows" abstracts from all of the building's

¹L. Wittgenstein, Philosophical Investigations, Trans. by G.E.M. Anscombe (Oxford: Basil Blackwell, 1967), p. 9.

²Ibid., p. 139.

properties save one.

Taken together, these sorts of considerations strongly suggest that there is not much sense to be made out of the notion that there might be an internal representation system in which images (which is to say, pictures in the mind or brain) are the vehicles of meaning - that is, in which entertaining an image or series of images is identical to thinking that such and such is the case.

Let us put the logical behaviourist point in the context of Shepard's theory. On the basis of his reaction-time findings, Shepard has argued that his subjects perform the similarity/dissimilarity calculation by rotating image-like or analogue structures in the brain into a position of mutual congruence. Suppose, then, that we ask the following question: Is it possible to represent the thought 'Object A can be rotated into congruence with Object B' in pictorial form? This, presumably, is the thought that mediates the problem solving activity of Shepard's subjects.

As a test of this, we might show a group of subjects (not necessarily Shepard's subjects, but subjects selected at random) a picture in which Object A (one of a pair of typical Shepard-type test objects) is superimposed over Object B. (In the picture, the superimposition is obvious because A is slightly smaller than B, is appropriately labelled, and is outlined in the colour blue, whereas B is outlined in the colour red). Would seeing this picture be equivalent, for the subjects, to having the thought in question? Would it convey to them the meaning 'Object A can be rotated into congruence with Object B'? Conceivably it could, but just as conceivably, it could communicate the thought

that 'there are two Objects, A and B, and A is outlined in blue and B is outlined in red.' Equally, it could communicate the thought that 'A is smaller than B', or that 'A is the same shape as B.'

It will be noted that this latter 'thought' is not equivalent to the thought 'A can be rotated into congruence with B.' Clearly, each implies the other (if two objects are the same shape then they can be rotated into congruence and vice versa, if they can be rotated into congruence then they are the same shape). But it is quite possible that a given individual - a small child, say - would think one thing without realizing the entailment to the other. In any case, the predicate 'can be rotated into congruence' conveys possibility or potentiality whereas the predicate 'are the same shape' is definitive - it states what is the case.

We could, of course, attempt to assist the subjects in their interpretation of the picture, in particular, to assist them in their ability to 'see' this potentiality by presenting them with two pictures - one in which objects A and B are simply side-by-side and one in which there is the superimposition of A over B. Or even better, we might elaborate on this scheme, by presenting the subjects with three pictures - the two pictures described above, plus another picture, placed between them, in which object A has been rotated a number of degrees about the picture plane axis toward object B. Would the subjects, if we ask them, "What do these pictures mean?" say they mean that object A can be rotated into congruence with object B? They might say this, or they simply might choose to describe each of the pictures separately. And what about a group comprised of subjects who have participated in

Shepard's experiments? What will they take the pictures to mean? Given their previous experimental experiences, it is perhaps quite likely that they will recognize them as displaying the potentiality for congruence. After all, they will have some notion that objects of this sort, displayed in a series, should be viewed, wholistically, as stages in a problem solving process.

Now I suspect that the logical behaviourist would take this thought experiment to illustrate the following: The object of the pictorial series presented to the subjects is not defined by any of the specifically pictorial properties displayed. Rather, it is carried by the description under which the series is intended. It may well be that a group of Shepard's subjects would say that seeing the pictorial series did, in point of fact, make them think of the possible congruence of A and B. But if this is so, it is not because of the series itself, but is because the subjects have been primed by their previous experience, to view it in a particular way. What counts here, the logical behaviourist will be concerned to point out, in terms of the communication of meaning - is not whether or how much the pictures resemble the object, but just how the perceivers 'read' the pictures.

Let me say, then, that I think the logical behaviourist argument regarding the nature of pictorial representation and more specifically regarding the difficulties inherent in the notion of 'representation-by-resemblance', is well-taken. In particular, it seems to call into question an idea about imagery which has played a long and influential role in a number of important empiricist theories of cognition. The idea in question is this: that thinking, problem solving, etcetera,

must, at rock-bottom, consist in imaging. The logical behaviourist argument shows quite clearly that this is not possible. If it is understood just how pictures and/or images represent, there can be no question that they could stand alone as the units of cognitive reference. Images/pictures require interpretation. And to the extent that this is so, there is really no sense to be made out of the idea that cognition, spatial or otherwise, can take place in a medium that is fundamentally, entirely non-discursive.

Having said this, however, I do not think that the logical behaviourist argument is absolutely devastating in its consequences for the image theory. More precisely, I do not think that we have here a refutation of the image theory per se but just perhaps one version of it. (This is the version maintained by the British Empiricists and, perhaps less frequently, by the sense datum philosophers, which makes images the sum and substance of thought.) That thinking cannot consist in imaging does not, I would argue, prove that images (or analogue structures) do not exist or that they are entirely without function. These are logically distinct possibilities. Thus it is still possible, I take it, to argue as Shepard does for a role for imagery in cognition so long as one does not make images the vehicle of thought in the tradition of J. Locke.

Roger Shepard has argued that people - his subjects in particular - solve the similarity/dissimilarity problem by performing a rotation of image-like or analogue structures in the brain into positions of congruence (or non-congruence, as the case may be). The logical behaviourist, on the strength of his image representation argument, might add a

valuable note of caution here. He might say to Shepard: It cannot be the case that this is all that is going on. The thinking, problem solving cannot consist in manipulating images or analogue structures. At some stage these structures and activities must be interpreted - they must come under a description - and at that stage what carries the weight of the cognition are the analogue structures together with the propositional intentions that interpret them. There must be a dual-code processing - discursive/propositional as well as analogue - going on.

5.5 The Images-as-Propositions Issue

This brings me to the second issue - the issue I have described as the 'images-as-propositions issue'. A number of logical behaviourists - Daniel Dennett among them - have been inclined to argue a more radical thesis about imagery. They have been inclined to argue that not only is it the case that images require propositional interpretation, but they are really nothing but propositions - just a way - a metaphorical way as Ryle would have it - of talking to ourselves. Thus, on this more radical thesis, the idea of imagery as some kind of cognitive representation is cast aside altogether.

Frequently, this thesis is argued on the basis of certain alleged facts about what it is to have an image experience as opposed to what it is to have a perceptual or physical object experience. The strategy of the comparison is to point out that, in the case of the latter, there is actually something 'there' to be perceived whereas, in the case of the former, there is not.

It is argued, for instance, that, usually, when we are imaging, we know what our mental image is an image of without the need to inspect it for clues. Even when an image just arises in the mind and cannot be recognized, no closer examination will provide clues to its identity; we have to wait until the name comes to us. In the extreme case of dreaming, we may 'recognize' a person even though his characteristics are entirely different from those possessed in real life. In perceiving, on the other hand, the object perceived may be identified gradually, by the collection of clues. Thus, 'having an image' of an object differs from contemplating either the object or a picture of it. The image is not a picture in a special private gallery. Psychologist John Heil marks the distinction between looking at something and imaging it in this way:

One cannot have an image of X without knowing that the image is of X. Not so for looking, for example. You are asked to imagine a house by a pond in rural England. You do this. Would it make sense to ask whether you are certain the image is of a house and not, say, of a paper maché mock-up; whether you are certain the scene is in England and not in Arkansas? Imagine Jimmy Carter. Now: how do you know it is Carter and not his twin or someone else disguised as Carter? This shows too, that images are not like pictures (where these and similar questions make sense).¹

Furthermore, it is argued that there are no factual discoveries to be made in imaging. Suppose, for instance, that we are looking at a

¹John Heil, in "Commentary on Kosslyn et al.: Demystifying Imagery," The Behavioral and Brain Sciences, vol. 2, 1979, p. 557.

photograph of a particular tiger in the London Zoo, or suppose that we are actually standing before the tiger's cage at the zoo. The perceptual experience either of the picture or of the actual tiger can be used to give new information. From this experience, it is possible to discover the number of stripes on the tiger's back. But, it is argued, the curious thing about imaging is that we cannot do this. Images are supposed to be pictures in the mind or in the brain (or in some kind of internal space) but, apparently, when we attempt to advert to them, to glean new information, no such information is forthcoming. Unless we already happen to know - as a matter of fact - how many stripes this particular tiger has, the mental image will not assist us. This suggests that images are not 'there' in the sense that pictures of striped tigers are there - they cannot be scanned for information.

Daniel Dennett takes this point to be absolutely crucial in its significance for the image theory. If, as the image theorists suggest, images are something other than or additional to propositions - a distinct kind of cognitive representation - then, he reasons, it should be possible to refer to them and in so doing obtain new (visual) factual information. We should, for instance, be able to count the number of stripes on the tiger's back. But, if as seems phenomenally to be the case, we cannot do this, then, surely, there is nothing to this idea of imagery in the first place. Surely there are no visual arrays before the mind's eye or the brain's eye or wherever, which can be scanned for information. Dennett proposes the striped tiger case as an 'acid test' for images, which, he claims, they ultimately fail. Dennett writes:

Consider the Tiger and his stripes. I can dream, imagine or see a striped tiger, but must the tiger I experience have a particular number of stripes? If seeing or imaging is having a mental image, then the image of the tiger must - obeying the rules of images in general - reveal a definite number of stripes showing, and one should be able to pin this down.... If, however, seeing or imaging has a descriptonal character, the question need have no definite answer.¹

Dennett's views are echoed by Zenon Pylyshyn. Pylyshyn argues that visual, factual discoveries cannot be made with imagery. He writes:

One misleading implication involved in using the imagery vocabulary is that what we retrieve from memory when we image, like what we receive from our sensory systems, is some sort of undifferentiated (or at least not fully interpreted) signal or pattern, a major part of which is simultaneously available. This pattern is subsequently scanned perceptually in order to obtain meaningful information regarding the presence of objects, attributes or relations.²

That this is misleading, Pylyshyn argues, can be seen by way of a consideration of the phenomenological facts. He invites us to suppose that we are recalling a scene from a party which we attended - sometimes, he says, the visual imagery is very clear and sometimes it

¹Daniel Dennett, "The Nature of Images and the Introspective Trap," in Imagery, N. Block (Ed.), 1981, op. cit., p. 55.

²Zenon Pylyshyn, "What the Mind's Eye Tells the Mind's Brain: A Critique of Mental Imagery," in Psychological Bulletin, Vol. 80, 1973, p. 10.

is not so clear. About this latter case he says:

When our recollections are vague, it is always in the sense that certain perceptual qualities or attributes are uncertain - not that there are geometrically definable pieces of a picture missing. This suggests that one's representation of a scene must contain already differentiated and interpreted perceptual aspects. In other words, the representation is far from being raw, and so to speak, in need of perceptual interpretation.¹

Thus, on the basis of these phenomenological observations, it is concluded that imaging an object is unlike perceiving an object because only in the latter case does the person stand in some relation to a second entity, (i.e., the actual object perceived). That is, imaging an object is not at all like seeing an object, but is more like the result of having recognized an object, that is, knowing what that object is. Part of the difficulty, as Ryle has argued, is that the grammatical expression 'S has a mental image' is misleading as to its logical form. There is, Ryle argues, a crucial logical difference between looking and imaging - looking is a relational expression and imaging is not.

To say S is looking at X, for instance, is to say that S is in ~~the~~ some relation to something, X. If it is true that S is looking at X, then it is also true that there is an X at which S is looking. This is not obviously so, however, for imaging. If S is imaging X, then S is not in some special relation of imaging to something X. Nor need it be

¹Ibid.

be true that there is an X that S is imaging. Imaging is logically intransitive, says Ryle, it is unlike looking and seeing - more like sitting and sleeping. To say that S imagines X is just to say that S is doing something, that S is imaging X-ly. The difference, then, between S imaging X and his imaging Y is not that there are two things, X and Y, to which S may be in some relation, but that in the one case S is doing something X-ly, while in the other case, he is doing something Y-ly. If we understand how the language functions - understand that there is a metaphor at work here - then we will not be tempted to think there is an image - a visual array - to which a kind of perceptual reference can be made, and from which perceptual information can be gleaned.

At bottom, I take it, people like Ryle, Dennett, Pylyshyn and Heil object to images because they believe them to be metaphysical curiosities. In particular, they claim to be incapable of making any sense out of the idea that images are pictures (in the mind, or in the brain, or wherever). They reason that if images really are in some sense pictures then they ought to meet certain minimal requirements. It ought to be possible (sometimes at least) to be puzzled as to their identity, as sometimes we are so puzzled about pictures, and it ought to be possible to make genuine discoveries of fact about our imagery. The truth is, they argue, that neither of these things seem to be possible. Images simply do not behave according to our expectations. Indeed, on a reasonably close consideration of our ordinary language conventions for talk of imagery, it becomes very clear, they argue, that it makes no sense to ask questions about identity, discovery,

etcetera. Apparently, we always know what we are imaging - we never have to take a second look so as to decide.

Clearly, what is at stake here, as far as the logical behaviourist is concerned, is the existence of imagery. That images fail to meet the requirements stipulated for them provides something like a demonstration that there are no images, or, at any rate, that what we call images are really propositions in disguise. The argument turns on two things: certain empirical claims about what happens (or fails to happen) when we are supposedly engaged in imaging and certain tests - adduced a priori for the existence of imagery.

Consider, first, the empirical claims. The logical behaviourist asserts that people's image experiences are never ambiguous. Intuitively, this sounds correct. While attempting to answer a question about Benjamin Franklin, I might well call to mind an image that, in truth, better depicts Abraham Lincoln or, for that matter, the current university librarian. But, for me, the referent of the image is still Franklin and I do not have to examine it sometime after the fact of my construction of it, to know that this is so. The referents of my imagery seem never to be problematic.

But, intuitions aside, do the logical behaviourists' empirical claims square with the facts? Is it ever the case that people have imagery which is informationally ambiguous? Apparently, this sometimes occurs. To take a particularly striking instance of this, in the book, The Mind of a Mnemonist,¹ A.R. Luria records a number of feats

¹A.R. Luria, The Mind of a Mnemonist (New York: Basic Books, 1968).

accomplished by a mnemonist referred to as S. S would be given verbal descriptions of complex scenes, often containing a number of individual items out of context, country scenes, say, with writing utensils in them, which he memorized imagistically. According to Luria, occasionally S could not remember some detail or other of the scene he had previously memorized. When this happened, S claimed that the context in which he encoded the detail was either too visually similar to provide sufficient contrast to see the item or it was too dark. S claimed that he would have to take a second look at the scene in order to discover the missing item, often he would simply visualize himself walking through it perhaps more than once. S reported:

I put the image of a pencil near a fence.... But what happened was that the image fused with that of the fence, and I walked right on past without noticing it. The same thing happened to the egg. I had put it up against a white wall and it blended in with the background. How could I possibly spot a white egg up against a white wall?... Sometimes I put a word in a dark place and have trouble seeing it so I go by.¹

And there are other less remarkable instances of this image ambiguity. Stephen Kosslyn (in press) describes an experiment in which subjects were given different descriptions (descriptions such as 'two overlapping rectangles' or 'four squares abutting a central square') of ambiguous patterns and were asked to image the pattern. All the subjects correctly answered questions put to them about the presence of

¹Ibid., p. 36.

parts in the imaged pattern (a rectangle, say) even though different parts were derived from different ways of parsing the figure. Sometimes, however, the subjects reported difficulty - claiming that they could not tell whether the embedded part was a square or a rectangle or some other geometric figure. This was particularly so when the part in question was small relative to the entire image. In addition, Kosslyn and Alper¹ had subjects image pairs of objects, one of which was to be imaged so small that it appeared as a speck in the image. Such patterns can be considered ambiguous in that a rabbit (one of the test objects utilized) with a speck on its back could be interpreted as a rabbit supporting a miniscule car, a miniscule typewriter, or a miniscule breadbox, and so on. And, in fact, the subjects reported difficulty in remembering which object it was they had imaged at the size level of a speck, while they had much less trouble remembering the larger 'host' object.² Finally, it has long been realized that any two-dimensional pattern is inherently ambiguous as a depiction of a three-dimensional scene, since many three-dimensional scenes could have given rise to the same two-dimensional projection. Pinker and Finke (in press) have shown that when subjects image a display of objects suspended in three-

¹Stephen Kosslyn and Steven Alper, "On the Pictorial Properties of Visual Images. Effects of Image Size on Memory for Words," in the Canadian Journal of Psychology, Vol. 31, 1977, pp. 32-40.

²See Kosslyn and Alper (1977) for details of various experimental controls supporting this conclusion and Janice Keenan and Robert Moore ("Memory for Images of Concealed Objects: A Re-examination of Neisser and Kerr," in the Journal of Experimental Psychology: Human Learning and Memory, 1979, Vol. 5, No. 4, pp. 374-385) for supporting evidence.

dimensional space, they can 'see' both the three-dimensional structure of the display and the two-dimensional geometric shape inherent in the frontal projection of the display. This corresponds to the well-known 'railroad tracks' ambiguity in perception: We can see that the tracks in front of us converge toward the horizon, but we can also see the same tracks parallel at every distance.

Thus, it would seem that the logical behaviourists have failed to get the facts straight regarding imagery. The data cited above indicate that, sometimes, particularly when the imagery in question is recalled rather than simply generated on the spot in response to a verbal description, questions of identity and interpretation do indeed arise. This means, of course, that contrary to the logical behaviourist pronouncement, images do not fail to meet the requirements stipulated for them. Images do appear to behave as one might expect if they were, in some sense, cognitive representations separate and distinct from propositions.

Equally, it would seem that the logical behaviourists have been engaged in the most flagrant of armchair psychology. After all, how does Dennett know that people can never make discoveries about imagery - that they can never count the number of stripes on the backs of their respective imaginary tigers? Has he ever actually put this to the test or sought out relevant data from psychological experiments of the appropriate types? Suppose, just for the sake of argument, that a 'striped tiger' experiment is conducted. Suppose that a group of subjects are shown a realistic photograph of a striped tiger, and then,

sometime later, asked to form a mental image and count the number of stripes on the imaginary tiger's back. Is it a priori obvious that they would be unable to do this - that, indeed, they would claim to be unable to make sense out of the instructions? Surely it is conceivable that some individuals would report that they had counted so many stripes, just as some others might well claim to be unable to do this. Not only is it conceivable, but the fact is that people actually do claim to be able to make factual discoveries on the basis of their image representations. And incidentally, more often than not, their discoveries turn out to be true. In the experimental series discussed earlier (in Chapter 4), Mendell and Janssen¹ found that when questioned as to the number of windows in their places of residence, the subjects reported calling up images of their respective houses, mentally walking around them, counting windows as they went. The control in this experiment is, of course, the reasonable expectation that most people do not know, in the sense of having the number at the tip of their tongues, how many windows there are in their houses.²

¹P.R. Mendell, "Retrieval and Representation in Long-Term Memory", op. cit., 1971 and W. Janssen, On the Nature of the Mental Image, op. cit., 1976.

²It will be noted that Dennett's thought experiment is somewhat different from the one I have adduced here. The difference is this: In the latter case the subjects are required to image a particular striped tiger (one presented to them earlier in a realistic photograph) whereas, in the former case - Dennett's case - the subjects are simply asked to call to mind a (which is to say, any) tiger. Now it seems to me very likely that Dennett's argument regarding the possibility of genuine discovery with imagery is best served by putting forward for our consideration his particular version of the thought experiment. Imagery called-up to represent a generic tiger is much more likely to be inspecific on the question of stripes than imagery called up to represent a particular tiger. It might, for instance, simply consist

Hitherto, I have been concerned to argue that, contrary to what the logical behaviourists apparently believe, images do not fail to meet the tests prescribed for them. In the process of this, I have more or less taken for granted the validity of these tests relative to the problem of the existence of images. I now want to consider whether or not these tests are indeed relevant to this problem.

Suppose, for the sake of argument, that it had turned out that the logical behaviourists were correct in their empirical assessment of imagery - people never encounter difficulty interpreting their images and they never report factual discoveries mediated by images. What would this prove? Would it show us something about the structure or function of images or about whether or not they really exist? Surely not, or, at least, not necessarily. Surely it is entirely possible that there might be image-like structures in the brain which are ambiguous (just as the duck/rabbit drawing used in psychology texts is ambiguous) but that other cognitive faculties - like propositions or subvocalizations to the effect 'I am seeing a duck' or 'I am seeing a

in the representation of a tiger's head, or may be of a tiger viewed at some considerable distance up a tree. In either case, there would be no stripes represented. Indeed, it is arguable, that in such a case, when called upon to think of a tiger, people would not have an image representation at all. In any case, the peculiarities of Dennett's thought experiment may go some of the way to explaining why Dennett seems so confident that people would never claim to be able to answer the 'number of stripes' question. Dennett seems to be assuming that a pictorial representation of a tiger per se must represent a determinate number of stripes. Normally this would be so, but I think we might want to grant that an out-of-focus photograph would constitute a tiger representation even though it is - because of the blurring - inspecific about the number of stripes the tiger has.

rabbit' - are activated in such a way that the subject is aware of only one reading of the ambiguous pattern. In fact, as any instructor of introductory psychology can attest, this frequently occurs in perception. Observers often have difficulty seeing the different interpretations of an ambiguous figure. Clearly, that this is so does not alter the fact of the objective existence of the figure or of its ambiguity. The observers see it, see even its ambiguity, but have access to and, therefore, report one reading only.

The point is that even if it is true that people are always perfectly clear about the identity of what they purport to image - that they can always give propositional representation to their image experiences so-called; and even if they cannot make factual discoveries on the basis of their imagery - they cannot, to use Dennett's example, count the number of stripes on their imaginary tigers' backs, this does not entail that an image is not 'there' in the sense of being present in the brain as a distinct kind of representational structure. It could simply mean that the image and the imaging activity are operative at a prior stage of cognition - a stage to which we do not always have direct access.

Alternatively, it could simply mean that the image is fleeting in the sense that there is not enough time to make discoveries of the 'stripes-on-the tiger's-back' kind. Images on television frequently display this characteristic. We might well see an image of a tiger in India on a documentary presentation, we might, indeed, notice that the tiger is striped, or has numerous stripes but because of the brevity of the duration of the image, be unable to count the precise number.

Alastair Hannay argues this point convincingly against Dennett's 'striped tiger' test. He writes,

... an inability to pin down the number of stripes on an imagined tiger may be due to the fleetingness, not the absence of the image ... the inherent instability of much visual imagery may be shared by some kinds of exemplary image - for example, a tiger may be represented by a changing assortment of dots and streaks - the numerical indeterminacy of the imagined tiger's stripes should seem no reason at all for denying that we see an image of it before the mind's eye.¹

It is worthy of note, in this connection, that the research findings of D.O. Hebb regarding the neurophysiological bases for perception and imagination suggest that images are quite likely to be 'fleeting' in the way that Hannay and I have indicated. The explanation for this, according to Hebb, has to do with the way in which the original perceptual encoding takes place. In the visual perception of a complex object (something like a striped tiger, say) 'first-order' cell assemblies in the brain are activated. They record a series of visual impressions or views of the object. (These views are determined by which parts of the object the eyes have fixated on during the perceptual sequence.) This information is then further analyzed and becomes integrated into the perception of a single object via the operation of 'second order' cell assemblies.

¹A. Hannay, Mental Images - A Defense (London, Unwin Brothers Ltd., 1971), p. 172.

Imagery, according to Hebb, involves the spontaneous reinstatement of the preceding perceptual activity. And depending on which hierarchical order of cell assemblies is activated, the resultant imagery varies in its vividness, sensory concreteness, and stability. Apparently, excitation of first order cell assemblies, produces vivid imagery but in bits and pieces only. The subject (if he or she is aware of it at all) experiences not a whole visual array, but 'flashes' of imagery, replications of the various views of the object as conveyed during the original perception. In the case of a tiger image, presumably, the subject might have access to the tiger's head, paws, tail and back, but none of these altogether. Hebb describes the fleetingness of the memory image in these terms:

Both the memory image and the eidetic image arise from perception. As we will see, this does not mean that the memory image is identical with perception (though eidetic imagery may be), but it does have implications that have not been recognized. The percept of any but the simplest object cannot be regarded as a static pattern of activity isomorphic with the perceived object but must be a sequentially organized or temporal pattern. The same statement applies to the memory.¹

So it would seem that the logical behaviourist tests are not definitive at all. It could be that people have images but because of the 'fleetingness characteristic' or the 'accessibility problem' or

¹D.O. Hebb, "Concerning Imagery," in Images, Perceptions and Knowledge, J.M. Nicholas (Ed.) (University of Western Ontario Series in Philosophy of Science, Boston: D. Reidel Co., 1974), p. 143.

some combination of these, they are unable to make use of their images in the way that they might make use of (or behave in respect of) a picture in a portrait gallery. This does not mean that there are no images, no analogue structures which convey spatial information literally rather than discursively, but just that there are no (portrait gallery) pictures in the brain or in the mind.

In one way or another, I believe that the logical behaviourists have been making too much of the need for interpretation when they express qualms over the idea that there are such things as imagistic thoughts. And, as I have indicated earlier, in connection with the discussion of the representation-by-resemblance issue, I have sympathies with their concerns. Whatever else Shepard may be saying in his analogue theory, he cannot be saying that analogue structures can stand on their own as the units of cognitive meaning. They do require interpretation. Nevertheless, the logical behaviourists seem often to go on to assume (or argue) that once we realize that pictures and other modes of representation require interpretation, the differences between the various modes must only be surface differences. In addition, it is often claimed that since all modes require interpretation, if they are to function symbolically, the underlying understanding of pictures and other modes must be propositional. This is, I take it, the basic structure of the inference which lies at the heart of the present issue. There are no images, it is concluded, there are just propositions.

But the fact that we must, at some stage, interpret pictures and interpret our images, does not mean that pictures and images really are just a propositional mode of representation. Images under

description are, after all, still images. Nor do I see why it is reasonable to dismiss a priori the idea that images may have a function.

Of course the case for this functional utility must be established on the basis of experimental investigation, but a priori one can point out two respects in which the construction of images might be helpful in cognition. First, if images represent spatial properties and relationships in an analogue form, then they may be more efficient for carrying out certain tasks than discursive, verbal representations. An example of such a task would be the similarity/dissimilarity assessment task required of Shepard's subjects. Second, an image might manifest emergent properties which could not be readily computed from the original fund of propositional information available to the subject. An example of this would be counting the number of windows in one's house by 'reading off' the information from an image. Clearly, if the subject does not happen to know as a matter of fact, how many windows there are in his house, he can only compute this on a pictorial basis. No other facts about his house will provide him with any basis for inferring window number.

The point is that images may integrate with discursive modes of internal representation. Recall, once again, the details of the Shepard experiments. There are two cognitive processes postulated by the proposed explanation of the results. In the first phase, a pair of images is constructed. In the second phase, a rotation to congruence or non-congruence takes place. The explanation thus implies (what common sense also suggests) that we have cognitive faculties that can

construct images which display the information that corresponding descriptions convey discursively. The reaction-time results demonstrate that having the information displayed as an image (i.e., in analogue form) facilitates performance in certain kinds of tasks. (In effect, using the imagery rather than a description enables the subjects to perform the task of shape assessment in parallel rather than in series: they can be dealing with object A and object B at the same time.)

5.6 The Infinite Regress Issue

There is, then, the third issue in the list (of logical behaviourist complaints) adduced earlier to be considered. This is the infinite regress issue. In general, it can be said that the issue focuses on the nature of cognitive explanation. And it is generated, say the logical behaviourists, by virtue of the fact that non-behaviourist philosophers and psychologists have insisted on postulating internal representations in the crucial explanatory role.

The problem is, they argue, that such representations simply duplicate the activity they were postulated to explain and, thus, they, in turn, require explanation. The argument given in support of this can be stated in abstract terms: nothing is intrinsically a representation of anything. Something is a representation only for or to someone. Thus, any representation or system of representation requires at least one user or interpreter of the representation who is external to it. And any such interpreter must have a variety of psychological or intentional traits - it must be capable of a variety of comprehension, and it must have beliefs and goals (so it can use the

representation to inform itself and thus assist it in achieving its goals). Such an interpreter is then a sort of homunculus - a little man inside the head who does everything the external man apparently does. He reads-off or hears the given information (presumably in former case, from some kind of internal viewing screen or, in the latter case, from some kind of inner voice) and then performs the requisite operation.

Suppose we say, for instance, as presumably the image theorist does, that we cannot account for spatial problem solving of a given type unless we suppose that the mind/brain is capable of representing the problematic situation to itself in imagistic terms. Does this explain the problem solving activity? On the contrary, it simply invites the retort - 'What good would image representation do us unless we have an inner eye to perceive it, and equally, how are we to explain its capacity for perception and problem solving?' The very notion of a 'mind's eye' seems to require a second processing system, or 'mind's eye's brain', to interpret information from the mind's eye, which, in turn, would require another eye to interpret the images projected onto this internal brain, and so on in an infinite regress. Daniel Dennett puts the problem of postulating images as cognitive explanations in these terms:

For an image to work as an image there must be a person (or analogue of a person) to see or observe it, to recognize or ascertain the qualities in virtue of which it is an image of something. Imagine a fool putting a television camera on his car and connecting it to a small receiver under the bonnet so the engine could 'see where it is going'.

The madness in this is that although an image has been provided, no provision has been made for anyone or anything analogous to a perceiver to watch the image. This makes it clear that if an image is to function as an element in perception, it will have to function as the raw material and not the end product, for if we suppose that the product of the perceptual process is an image, we shall have to design a perceiver-analogue to sit in front of the image, and yet another to sit in front of the image which is the end product of perception in the perceiver analogue and so on ad infinitum.¹

And consider Wittgenstein's statement of the infinite regress problem relative to the postulation of imagery:

How is he to know what colour he is to pick out when he hears 'red'? Quite simple: he is to take the colour whose image occurs to him when he hears the word. But how is he to know which colour it is 'whose image occurs to him'? Is a further criterion needed for that?²

Here the original puzzle has to do with how we know to what colour the word 'red' refers. The alleged explanation (on the traditional empiricist account of language use) is that we find this out by having an image of 'red'. But Wittgenstein's argument then runs, if we need a criterion to determine to what colour the word refers, we should equally need a criterion to determine to what colour the image refers. And, similarly, it might be added, we should need a further criterion to

¹Daniel Dennett, "The Nature of Images and the Introspective Trap," in Imagery, Ned Block (Ed.), op. cit., p. 52.

²Wittgenstein, Philosophical Investigations, op. cit., p. 239.

determine to what colour this new criterion applies, and so on. So, in effect, we could never use the criterion. To point this out underlines the unsatisfactory character of the original explanation and makes it perfectly clear, too, that we cannot evade the difficulty by introducing a third, fourth, or fifth criterion into the story.

The gist of the infinite regress argument, I take it, then, is this: If the fact that people solve spatial problems of the similarity/dissimilarity type when they are seated in a psychology laboratory with the test objects physically present to them is somehow in need of explanation, that explanation cannot be furnished by simply arguing that people can generate internal replicas of the objects in question and so simulate the problematic situation. On the contrary, if the former ability/activity is in need of explanation, then so also is the latter ability/activity. Retreating to the 'internal form' makes no substantial difference to the problem solving. Thus, it is concluded that postulating analogue structures as Shepard, for instance does, does not provide an explanation, it only pushes that explanation back a step.

Now it seems to me that there are three considerations that could well mitigate the force of the infinite regress argument as far as Shepard's image theory goes. The first of these has to do with whether or not the first step in the alleged regress does in fact duplicate in its entirety the activity it is designed to explain (and hence invite explanation itself). Shepard has argued that the problem solving activity of his subjects goes forward on the basis of the generation in the brain of structures which are analogues of the

physical objects in question. One could formulate this more formally in terms of the general pronouncement: Spatial problem solving of type 'X' requires the utilization of internal analogue structures. But, it will be recalled, there is more to Shepard's story than this. Shepard stipulates both structures and processes which operate on and manipulate these structures. Not only are there analogue structures in the brain, but, according to Shepard, these structures undergo a process of rotation - analogous to the process which would, presumably, take place physically/behaviourally if the subjects were free (as they are not in the experiment) to handle the test objects and move them into positions of relative congruence. Thus what happens in the internal forum is in no sense a replication of the activity in need of explanation. On the contrary, the 'explanatory' activity includes a further, and, as it happens, crucial, processing step. There is a genuine causal explanation here: people solve spatial problems of the given type because the rotation enables them to do this. Certainly there is a regress of explanation here in the trivial sense that there is in any explanation - a problematic situation or term has been explained in terms of something else. But it is, I take it, a regress of one stage only.

Secondly, there is no reason to assume that if the similarity/dissimilarity calculation is or must be preceded by the generation and rotation of analogue structures in the brain, then the calculation of similarity/dissimilarity in the case of the internal analogue structures themselves requires the generation of further analogue structures and so on. On the contrary, all we are asked to assume is that the

nervous system of the human organism is 'constructed' or 'wired' in such a way as to accomplish the relevant rotation operation.

This is, I take it, essentially the same argument that Jerry Fodor has put forward in order to defend imagistic explanation from the infinite regress objection. Fodor characterizes the objection in this way,

Having images is supposed to be part of the perceptual process. But now, if images themselves have to be perceived (scanned, etc.) to recover the information they contain, then surely we have taken the first step in a regress which will eventually require the postulation of images without number and endless perceivers and look at them.¹

To this Fodor replies:

This is, however, a bad argument. It assumes, quite without justification, that if recovering information from the external environment requires having an image, recovering information from an image requires having an image too. But why should we assume that.²

If we consider just how the average computer operates - just how it eventually comes to cash-out its own internal representation structures - we can, says Fodor, learn a valuable lesson. The lesson has to do with just how human cognitive representations such as images might get 'cashed-out' without any deficit of explanation. Fodor gives a

¹Jerry Fodor, The Language of Thought (Thos. Crowell and Co. Inc., 1975), p. 19.

²Ibid., p. 19.

rough sketch of computer operation. Real computers characteristically use at least two different languages: an input/output language in which they communicate with their environment and a machine language in which they talk to themselves (i.e., in which they run their computations). 'Compilers' mediate between the two languages in effect by specifying bi-conditionals whose left-hand side is a formula in the input/output code and whose right-hand side is a formula in the machine code. Such bi-conditionals are, to all intents and purposes, representations of truth conditions for formulae in the input/output language, and the ability of the machine to use that language depends on the availability of those definitions.

Fodor's point is that though the machine must have a compiler if it is to use the input/output language, it doesn't also need a compiler for the machine language. What avoids an infinite regression of compilers and, presumably, what would avoid an infinite regression of images, is the fact that the machine or the brain is built to use the machine - or image - language. Writes Fodor,

Roughly, the machine language differs from the input/output language in that its formulae correspond directly to computationally relevant physical states and operations of the machine. The physics of the machine thus guarantees that the sequences of state and operations it runs through in the course of its computations respect the semantic constraints on formulae in its internal language. What takes the place of a truth definition for the machine language is simply the engineering principles which guarantee this correspondence.¹

¹Ibid., p. 66.

One might say, then, the cognitive explanation by way of analogue structures and processes in the brain, is a 'privileged' kind of explanation - just as, in religious contexts, explanation of the existence of the universe, by way of the postulation of an Unmoved Mover is a 'privileged' kind of explanation. God is, so to speak, a bottom-line in the causal chain - it makes no sense to ask on what (causal) basis God exists, it is simply in the nature of God that He does. Equally, it is in the physical-neurological nature of the human brain that similarity/dissimilarity assessments can be made on the basis of the generation and rotation of analogue structures - no further explanation - no further postulation - is invited or required.

The third consideration which, it seems to me, is relevant to the discussion of the infinite regress problem and its consequences for Shepard's work is this: Shepard has put forward for our consideration, a straightforward empirical hypothesis - problem solving of type 'X' goes forward on the basis of the generation and rotation of analogue structures in the brain. The infinite regress does not apply in the case of assertions of this sort, any more than there is a regress involved in asserting that every happy marriage is preceded by a happy engagement. This latter is simply a sociological hypothesis to be examined as such. A person can put forward this hypothesis without being at all committed to asserting that every happy engagement is preceded by a previous happy engagement. Similarly, to assert that spatial problem solving of type 'X' is preceded by 'analogue' brain activity, is to propose a physiological hypothesis which could be criticized only by making observations and seeing whether we can

discover cases where in fact spatial problem solving of the given type occurs without the requisite analogue activity. A person, like Shepard, who says that in fact such problem solving is always so preceded is not at all compelled to hold that every instance of analogue activity is in turn preceded by another instance of analogue activity. An infinite regress argument, to put the matter generally, has no applicability to the straightforward empirical assertion that every A is preceded by a B.

The regress argument would have applicability only in circumstances where the 'explanatory assertion' purports to be conceptual rather than straightforwardly empirical/causal. Let me explain. Suppose that Shepard had argued that being preceded by the generation and rotation of analogue structures in the brain is what makes a given activity an instance of problem solving of type X. Roughly, the thesis here is that the problem solving activity is constituted by its particular etiology. This assertion, the logical behaviourist might well say, is problematic. In particular, it does not provide an explanation of anything. For on this account, one cannot possibly know that a given piece of problem solving behaviour is in fact the genuine article without first knowing that it is preceded by analogue activity. But, then, according to the regress argument, it must equally be the case that the problem solving quality of this activity is constituted by its relation to previous analogue activity and so on ad infinitum. So we are never in a position to discover whether an act is or is not spatial problem solving of the given type, although this is just what the alleged explanation set out to tell us how to do.

Ryle and, I take it, the other logical behaviourists seem to harbour an objection in principle to imagery simply because, more often than not, when philosophers and psychologists have postulated such entities, they have cast them (incorrectly) in the conceptual role. Which is to say that they have sought to use imagery to explain what spatial problem solving, or intelligence, or thought etcetera is. But conceptual problems as Ryle rightly insists, cannot be dealt with by postulating entities and/or processes in an internal forum. Problem solving behaviour is not what it is because it has the kind of etiology it has. Indeed, the physical causality of the problem solving behaviour is beside the point here. That the behaviour of Shepard's subjects constitutes problem solving behaviour of type 'X' is decided by extra-causal considerations - specifically, that the subjects are participating in a psychology experiment, that they are seated in a laboratory, and that they are pushing buttons, pulling levers, etc. indicating, when successful, answers which the psychologist conducting the experiments has already decided are correct. The satisfaction of these (behavioural/contextual) conditions is what makes this an instance of problem solving of type 'X' - these conditions define the nature of the act.¹

¹Richard Rorty explains the origin of the Rylean objection (paranoia?) to (about) intellectualist theorizing at some length in his book, Philosophy and the Mirror of Nature (Princeton University Press, 1979). Rorty argues that because philosophers and psychologists have so often been guilty of offering causal explanations in response to conceptual problems and vice versa, Ryle and company have come, over time, to adopt an 'across the board' reaction to intellectualist theorizing of any kind. In particular, they see the dreaded infinite regress problem, cropping up whenever internal entities, states (such as images) etc. are proposed. Rorty writes, "The central point is that explanatory entities postulated by psychologists reduplicate problems in the explananda only when these problems are bad problems anyway -

The important thing to bear in mind for the purpose of assessing Shepard's image theory is that conceptual explanations and causal explanations are not in competition. Each is an acceptable kind of explanation. In particular, it is perfectly acceptable to seek a causal explanation for the behaviour of Shepard's subjects (or anyone, for that matter, engaged in similarity/dissimilarity calculations of appropriate sort), so long as one does not make the mistake of attempting to answer the conceptual/definitional question, to explicate how to identify spatial problem solving with this explanation. Shepard's analogue theory, I take it, does not offend against Ryle's (conceptual/causal) distinction. The analogue theory is explanatory for the purposes it was originally designed.

5.7 The Privacy Issue

The fourth and final issue that logical behaviourists adduce in connection with imagery is the privacy issue. The logical behaviourists insist that however plausible for empirical reasons an image based account of cognition may be (even if, as in the case of Shepard's experiments, such an account appears to explain an otherwise very puzzling reaction-time pattern), it must be wrong. It is in the nature of the case, they argue, that our experience, naming, and use of images

for example, "How is recognition possible?" Philosophers like Malcolm and Ryle are accustomed to bad philosophical answers to bad philosophical questions: "How is motion possible? - as the actualization of potential qua potential;" "Why does nature follow laws? - because of God's benevolence and omnipotence." Consequently, they tend to see such questions lurking behind even quite specific and limited research programs." p. 239.

is idiosyncratic. Because of their highly individual and concrete nature, and because they presumably subsist somewhere/somehow in the internal forum, images do not, indeed, cannot conform to a socially based set of rules and conventions. And, in principle, there is nothing to prevent their being used entirely at random.

At bottom, I take it, the logical behaviourists object to what they take to be the 'private language' implications of postulating imagery. It is thought that by countenancing imagery one is as well countenancing - even endorsing - the possibility of a private language. And this possibility, the logical behaviourists, following Wittgenstein, reject a priori.

Wittgenstein, on my reading of him in the Philosophical Investigations, is basically concerned to show that no definite sense attaches to the notion of a term in a private language being used coherently - as opposed, that is, to being used at random. Wittgenstein has, in this respect, two ways of characterizing a private language: either as one whose terms refer to things that only its speaker can experience or as a language for the applicability of whose terms there exist no public criteria or rules or conventions. For Wittgenstein's purposes (which I take to be that of attacking the idea of a sense datum language) these two formulations come to pretty much the same thing. If I am the only one who can know what a term like 'image of object A' refers to then, clearly, the conventions for applying that term cannot be public. For, by hypothesis, only I could tell when the conventions are satisfied; only I know whether a certain event is of the kind that falls under the conventions. And in such a situation, says Wittgenstein,

where there is no possibility of an objective, public check, there is really no difference between getting the use of the term 'image of object A' right and getting it wrong - no difference between obeying the conventions for the use of the term and failing to obey them.

Now this raises certain questions in relation to the topic of present concern. Do Shepard's 'mental images' and 'mental rotations' constitute a kind of private language as judged by either or both of the standards of privacy adduced by Wittgenstein? More generally, we might ask, does Shepard, in his formulation of the image rotation theory, make the mistake of postulating states and processes for which there is in principle no possibility of establishing grounds for their coherent use? And, do the terms 'mental image' and 'image rotation' as they are utilized in Shepard's theory, refer to things and events which are essentially and irrevocably private?

With these questions in mind, let us review, briefly, the facts of the case. It is a general assumption of Roger Shepard's image rotation theory that the image rotations which people report are experienced brain states of a particular kind. They are states with which people have an active relationship. People monitor the process of a rotation and so come to solve the shape assessment problem in the length of time it takes for the rotation (into congruence or non-congruence) to be completed.

Are these monitored states private? De facto they certainly seem to be. Mental images in general and image rotations in particular are internal - we cannot put them on a table, so to speak, for all to see. This means that we cannot provide an ostensive definition for an

instance of image rotation as we can for the dog 'Fido'. Image rotations cannot be seen or experienced by other people with the 'outer eye'.

But privacy, surely, is a matter of degree, not of all or none. And just because a given object or event is not ostensively definable, does not mean that it cannot be so defined in certain circumstances. Consider, once again, the situation in which a companion reports that he is holding several coins in the right-hand pocket of his jacket. Are the coins private to this individual? Certainly, they have all the earmarks of being so. No one but the individual making the report can see or have any physical contact with the coins and so as external observers we must take it more or less on faith that our companion knows what he is talking about and that he is not intending to deceive us. However, we could if we were inclined, require or even force our companion to let us take a look and see whether or not there are any coins in the pocket to be handled. Coins are the kinds of things that can in principle be brought into the public forum and be subjected to public inspection.

Now it seems to me that Shepard's image rotations are very like the coins in our companion's pocket, which is to say that they are private as a matter of fact but not necessarily or irrevocably so. Let me explain. It is a crucial feature of Shepard's image rotation theory that it has to do, ultimately, with neurophysiological states and events. The image structures and image processes are actual physical states and processes occurring in/on a physical organ - the human brain - which people are able to experience. Such things are, I take

it, public, at least in principle. Of course, it is true that such structures and processes have not yet been identified in the brain through neurophysiological investigation. But this simply reflects current limitations in our ability, technically, to deal with, examine, and map the brain. The important point is that, if Shepard's theory is true, there can be some ~~day~~ an empirical discovery to attest to its truth - a discovery of some kind of neurophysiological state and/or event which has analogue properties. And, if so, such a discovery will be open to public examination, public discussion and image rotations will then be, like the revealed coins, ostensibly definable.¹

Of course it is one thing to argue that other people can identify and observe the image rotation activity in the brain tissue of an individual reporting image rotation but quite another to argue that other people can actually experience - that they can actually see images of Shepard-type objects rotating before their mind's eyes. In the case of the hidden coins, it is not only possible for other people to see them, but they can actually replicate the reported phenomenological experience by placing their hands into the jacket pocket and grasping the coins. They can, that is, have the experience of handling the coins, just as the original coin reporter did. Now I see no reason why

¹Of course, observation of brain activity is very likely to be a complicated affair and it is probably reasonable to expect that 'observing' an image rotation in the cerebral cortex of an individual reporting it, will not be exactly like having a mind's eye experience of rotation or observing a rotation in a movie. After all, the brain and movie screens are radically different kinds of substance. And current research seems to indicate in fact that 'observing' mental imagery will be more a matter of tracing electrical impulses with the aid of instrumentation of some description than the straight-forward seeing of shapes in rotation with the naked eye.

this phenomenological replication could not be a possibility for image rotation as well. If it is true that neuroscientists can ultimately identify, in the brain, image rotation, then surely it is possible that such brain activity could be generated or simulated in the brains of other people (à la Wilder Penfield?). In this case, we might say that not only can people see and verify image rotation as it occurs in an individual reporting it, but they can also experience the same image rotation themselves, by way of a replication of the appropriate brain activity. And we might say to them, (though without literally pointing) "This is image rotation." I would argue, therefore, that judged by the first standard of privacy adduced by Wittgenstein, image rotations are not private. In principle they are open to the scrutiny and experience of others.

Now the question is, what can we say about the second standard of privacy adduced by Wittgenstein? This is the standard which has to do with public conventions. Are there currently in place standards or rules or language games to use Wittgenstein's description, for the correct use of verbal reports of image rotations?

The first thing that ought to be acknowledged here is that image rotations (and, indeed, image experiences of any sort) would appear to be quite unlike pains or itches in that there are no obvious primitive behavioural manifestations of them. (People do not grasp a leg and wince as they do with a cramp, when they are imaging.) There are certain behavioural signs which might be observed, such as an individual's having a pensive expression and gazing towards an indeterminate and intrinsically unimportant area of space but this hardly

distinguishes between the individual's reciting a poem to himself or day-dreaming (linguistically) and imaging a rotation nor could these behavioural signs be intimately involved in teaching or justifying the employment of the concept of image rotation. There are no natural behavioural expressions of having an image rotation for "I am having an image rotation" to be a conventionalized extension of.

Having said this, however, it seems to me that there are conceptual standards which might enable the external observer to judge the sense, reasonableness and even accuracy of given verbal reports of image rotation. Consider the following set of reports: A mathematician claims that he is rotating mental images in order to solve a problem of trigonometry. A small child (of three or four years) claims that he is mentally rotating images and when questioned about this, provides a demonstration of what a rotation is like by running his hand in a continuous direction over the surface of a table. A group of students are observing a physical demonstration of rotation involving three-dimensional models of Shepard-type objects and they report that they are seeing mental images of these objects rotating before their mind's eyes.

What is odd about these image rotation reports? The first report and the last report of image rotation have been made in inappropriate contexts. There is no connection of a problem solving kind between trigonometry and image rotation and so we might reasonably conclude that the mathematician must believe that image rotation is something which it is not. And whatever the students may believe, when they are sitting, eyes open - before a physical demonstration of objects in

rotation, they are seeing the rotation, not imagining it. The child's report, on the other hand, is a more straightforward case of misunderstanding - considerations of contexts do not lead us to this conclusion, the child's simulation of rotation does. Image rotation is not anything like continuous movement in a straight line, and if the child thinks that it is, he does not know what a rotation is and so cannot reasonably be construed to be making a report of it.

I would argue, therefore, that the private language argument cannot really be directed against the sort of theory that Shepard has been advocating. Talk of Shepard's image rotations is no more mysterious, no more ungoverned and random than our companion's talk of the coins in his pocket. There are standards in place which enable us to make judgments about reports of image rotation. People cannot just say anything about image rotation and get away with it. People who demonstrate (by the contexts in which they make their reports, or by attempted simulations) that they do not know what image rotation is, cannot be reporting it.

5.8 Summary

Let me summarize the argument of this chapter as it has advanced thus far. The four issues which have been adduced as constituting the logical behaviourist case against Shepard's theory have been found to be either indecisive against that theory or, worse, wrongly adduced in the first place. In the case of the first issue - the representation-by-resemblance issue - it has been argued that Shepard's theory is less extreme than the more conventional image theories in that Shepard has

made no attempt to make imaging the sum and substance of spatial thought. In particular, Shepard leaves open the possibility for a dual-coding model of cognition. On such a model, there is, in addition to the postulated image system, a linguistic system and these two systems are presumed to be functionally inter-dependent.

In the case of the second issue - the images-as-proposition issue - it has been argued that the possibility of 'factual discovery' is not a test that image systems always must fail (c.f. the 'number of windows case') and not a definitive test anyway for the existence or non-existence of an image-like system of cognitive representation. The explanation for the alleged fact that people are unable to discover the number of stripes on an imaginary tiger's back may well point to some peculiarity in the mechanics of image processing rather than the non-existence of image-like or analogue structures in the brain.

In the case of the third issue - the infinite regress issue - it has been argued that Shepard's image theory provides a genuine causal explanation for the behavioural phenomenon in question and that, consequently, no regress of explanation arises. People solve the similarity/dissimilarity problem in the way that they do because (causally because) image-like structures in the brain have achieved mutual congruence or non-congruence. In addition, it has been noted that no attempt is made by Shepard to provide by way of the postulated analogue structures and processes, a conceptual account of what spatial problem solving per se is.

And finally, it has been argued that the privacy issue, so called, is (or would be) inappropriately applied to Shepard's theory.

Shepard's mental images and image rotations are experienced states and processes of a physical organ, specifically the human brain and, as such, they are public, at least in principle. Additionally, it has been noted that there are public conventions which establish the sense and reasonableness of given image rotation reports.

I would argue, therefore, that Shepard's theory does not break any of the established philosophical rules about cognitive explanation. Shepard does not propose that spatial cognition should consist - exclusively - in imaging, nor does he purport to tell us what spatial cognition essentially is, nor, for that matter, are his image structures and processes irrevocably private. They only seem to be so because of the relatively primitive state of our knowledge of neurophysiology. The truth of the matter is that Shepard is an image theorist in a new sense and the standards by which it might have been appropriate to judge and criticize the theories of Locke, Hume or Bertrand Russell are inappropriate here.

So the questions with which this chapter began remain. What is wrong with cognitive explanations couched in terms of image rotation? And what sense can we make out of the decidedly negative attitude displayed by Shepard's commentators and critics? If Shepard's theory provides a causal account of the reaction-time phenomenon, and if there is nothing in principle wrong with such an account and if, as I have argued at the outset of this chapter, alternative, linguistic theories regarding the reaction-time phenomenon seem to be postulated with

difficulty and at considerable theoretical cost, then there would seem to be no good reason not to take Shepard seriously and, indeed, to fail to do so would seem to betoken a deep-seated paranoia regarding the matter.

H.H. Price in his Thinking and Experience¹ has argued that this paranoia arises as a consequence of the weak imaging ability of most, if not all, intellectuals. For them, according to Price, what is usually called 'imaging' by the lay public simply fails to give rise (in intellectuals) to anything that could properly be called a 'visual' or even an incipiently or quasi-visual, experience. Price describes the situation re image experience in these terms:

It begins to look as if this extreme view about the total irrelevance of images in thinking were just academic in the bad sense of that word: a superstition engendered in the minds of highly intellectual persons by their ignorance of what happens in ordinary unintellectual mankind.... Ask the ordinary man (not a philosopher) what he means by the word 'butterfly' and he will very likely have a visual image representing the Cabbage White. Say to him 'there is an elephant coming down the street' and he will very likely have a visual image of this unlikely spectacle, perhaps a vivid and detailed one.²

¹H.H. Price, Thinking and Experience (Hutchinson and Co. Ltd., London, 1969).

²Ibid., p. 237.

5.9 Attitudes to Imagery

There is some empirical support for Price's postulation as to the likely distribution of image experience across individuals. Evidently, not everyone has equivalent mind's eye sight. Sir Francis Galton discovered this to be the case when he conducted his statistical investigation of visual imagery reports. Galton proceeded by sending a questionnaire to prominent scientists and thinkers. Among other things, he asked his subjects to describe the vividness of their mental imagery as they attempted to recall, as clearly as possible, their morning's breakfast table with all its contents. A number of the would-be respondents to this questionnaire evinced genuine surprise that anyone would seriously ask questions about the size, shape, faintness or vividness, of given (supposed) image-objects, when, so they said, they did not now (or indeed ever) experience such objects. They could recall 'facts' - which is to say, propositions, about the breakfast table (that it had a table cloth of such-and-such a colour on it, that a tea pot was sitting in the middle, etc.) but nothing else.¹

G.H. Betts conducted a similar course of investigation with similar results.² He found that some of his subjects reported imagery experience, while some did not. Betts also found that depending on which 'camp' of imaging ability the individual subjects fell into ('the

¹Francis Galton, Inquiries into Human Faculty and Development (London, 1883).

²G.H. Betts, "The Distribution and Function of Mental Imagery," Columbia University Teachers College Contributions to Education, 1909, No. 26.

good visualizers' or the 'poor imagers' as Betts described them) they often held a corresponding prejudice. The subjects who reported visual imagery tended to react with disbelief or even pity when told that some people think mainly in words. Conversely, the subjects who claimed to be without the image experience, seemed to be puzzled as to how it might be possible to form visual images and, once formed, how any kind of rational thinking is likely to take place.

Anne Roe used interviews to find out the experienced representation of thought in various scientists.¹ Her results indicated that about one-third were visualizers, one-third were verbalizers, and about one-quarter were 'imageless' in that they just knew something was going on without being able to qualify the nature of their representations. (The rest were mixed.) Significantly, Roe found more visualizers among physical scientists and more verbalizers among social scientists.

It could be, then, that the linguistic theorists are like Galton and Betts' recalcitrant subjects or Roe's social scientists - bereft of the imagery experience. And when confronted with the necessity of solving spatial problems, they generally find themselves engaged in an abbreviated form of private dialectic or, just as likely, they find themselves having no problem solving experience of any kind. This, at any rate, appears to be the phenomenology behind Daniel Dennett's attempt to tell the image introspector participating in Shepard's experiments what his problem solving experience is really like. He is

¹Anne Roe, "A Study of Imagery in Research Scientists," Journal of Personality, Vol. 19, 1951, pp. 459-470.

not having an experience of images rotating in a mind-space, Dennett insists, he only thinks he is. The following passage is clearly autobiographical and it betrays (though I am sure Dennett would be shocked to hear it) a phenomenological predisposition to the linguistic theory. Dennett writes,

Aren't we directly aware of an image rotating in phenomenal space in this instance? No. And that much, I think, you can quickly ascertain to your own satisfaction. For isn't it the case that if you attend to your experience more closely when you say you rotate the image you find that it moves in discrete jumps - it flicks through a series of orientations. You cannot gradually speed up or slow down the rotation, can you? But now, 'look' again. Isn't it really that these discrete steps are discrete propositional episodes?¹

¹Daniel Dennett, *Brainstorms*, op. cit., p. 168. Dennett may well be shocked to hear that a phenomenological prejudice has been ascribed to him, but it is interesting to note that elsewhere (in "The Nature of Images and the Introspective Trap," *Imagery*, N. Block (Ed.), 1981, op. cit., Dennett has himself had occasion to make a case for the influential role that phenomenological considerations can play in the development of a theory of cognition. In particular, Dennett refers to the 'introspective trap' so-called, to which, he tells us, non-philosophers have fallen prey, and he attempts to explain the development of the image theory on this basis. He writes, "Introspection is often held to tell us that consciousness is filled with a variety of peculiar objects and qualities that cannot be accounted for by a purely physical theory of mind." (Dennett, 1981, op. cit., p. 52). I am simply suggesting that it is not unreasonable to expect that the introspective trap could cut both ways - that philosophers may likewise be influenced by phenomenological considerations. They experience (or make reference to) no images (or precious few) during problem solving sequences, and so find the suggestion that images have a significant role to play in cognition quite incredible. Just as the layman's natural inclination (so Dennett insists) is toward the image theory, so too the philosopher's natural inclination is likely to be toward the linguistic theory. This supposition, it seems to me, is entirely consistent with the strategy of Dennett's own 'introspective trap' account of things.

No doubt this will seem a rather harsh judgment of the ultimate motivation of the linguistic theorists (in particular and most philosophers and psychologists in general). After all, to say that they have a phenomenological predisposition to the linguistic theory, or to put this the other way around, that they have a phenomenological aversion to the image theory, is to ascribe to them nothing less than the attitude of the proverbial 'dog in the manger'. Indeed, the operative suggestion here seems to be that, motivationally, the linguistic theorists are not unlike the puritanical Victorian matrons who felt compelled to describe sexual enjoyment as base, decadent, and even unnatural, simply because they did not find sex to be enjoyable. And the evidence for this attitude - the one revealing passage from Dennett and the fact that some individuals are weak imagers and have a corresponding prejudice about the nature of cognitive representation - is not exactly overwhelming.

But there is, I think, more substance to this explanation by phenomenological predisposition/aversion than might be evident at first glance. The fact is that there appears to be good reason to suppose that the linguistic theorists are quite likely to fall into Betts' category of 'weak imagers'. The evidence for this comes from psychological research into the presence, in some individuals, of the phenomenon known to psychologists as eidetic imagery and more popularly known as photographic memory.

The individuals possessed of such memory experience visual imagery which is percept-like in its vividness and are able to read-off from that imagery, details which, apparently, are lost to normal imagers.

A particularly striking instance of this phenomenon was recorded in experiments conducted by Gordon Allport in 1924.¹ A group of thirty eleven-year old children were asked to examine a picture depicting a fairly complex European street scene for a period of nine seconds. Later, looking at an empty white screen, on which they were told to project their imagery (if they could), all but a few of the children were able to glean details from the picture as though it were still present. They could count the number of windows on a house depicted in the background of the picture, and the number of cans on the milk cart depicted in the foreground. And, remarkably, three of the children were able to spell the word 'Gartenwirtschaft' either forward or backward even though they knew no German and the word was, to say the least, hardly noteworthy printed above a shop in a scene filled with people, animals and activities.

Another striking instance of this phenomenon has been recorded by Stromeyer and Psotka in 1970.² The Stromeyer and Psotka experiments involved the use of a computer generated pattern which was cut down the middle and presented to the subject (in this case a young woman) in two pieces. The subject was asked to recall the pattern piece which was presented to her left eye several hours earlier while the matching pattern piece was simultaneously presented to her right eye. A central three-dimensional square which is evident to people who view both

¹G.W. Allport, "Eidetic Imagery," British Journal of Psychology, Vol. 15, 1924, pp. 99-110.

²C.F. Stromeyer and J. Psotka, "The Detailed Texture of Eidetic Images," Nature, 1970, pp. 346-349.

patterns simultaneously, was clearly visible to the subject.

Now the significance of the photographic memory phenomenon in the context of the present discussion is not so much that it occurs but the peculiar distribution of its occurrence across subjects. On the evidence available so far, a pattern is clearly decipherable. Eidetic experience is correlated with age: The older a given subject is the less likely he or she is to have the eidetic experience, conversely, the younger a given subject is the more likely he or she is to have the eidetic experience. Both Teasdale (1934)¹ and Morsh and Abbott (1945)² have shown this trend for random samples of subjects between the ages of ten and nineteen years. Teasdale found 12.5% with eidetic imagery in their eleventh year, 8.3% in their twelfth year, 5.8% in their thirteenth year, reducing to only 2.1% among those in their fourteenth year. Eidetic imagery does occur in adults but is very rare. Purdy (1936)³ reported an adult subject with eidetic imagery in the modalities of sound, smell and touch in addition to sight, and more recently, Peter Sheehan (1968)⁴ reported an eidetic twenty-one year old male medical student who also appeared to possess vivid auditory imagery of an eidetic type. At the other end of the scale, six and seven year old

¹H.H. Teasdale, "A Quantitative Study of Eidetic Imagery," British Journal of Educational Psychology, Vol. 4, 1934, pp. 56-74.

²J.E. Morsh and H.D. Abbott, "An Investigation of After-Images," Journal of Comparative Psychology, Vol. 38, 1945, pp. 47-63.

³D.M. Purdy, "Eidetic Imagery and Plasticity of Perception," Journal of General Psychology, Vol. 15, 1936, pp. 437-453.

⁴P.W. Sheehan, "Color Response to the TAT: An Instance of Eidetic Imagery?" Journal of Psychology, Vol. 68, 1968, pp. 203-209.

children seem almost always to have the eidetic experience in some degree.¹

Now what could account for this age-image correlation? Why should the ability to experience vivid percept-like imagery occur so rarely among adults? Apparently most, if not all, of us had this ability in some degree during our childhood. But, somehow, in the gradual transition from childhood to adulthood, we have lost it. Could it be that as we grow older, we get 'out of practice' with images?

An impressive list of psychologists² have argued that this is precisely what happens. The suppression of the eidetic image experience occurs as a consequence of the fact that as we grow with adulthood in a modern industrialized society, verbal modes of encoding take precedence over the image modes of early childhood. We are taught language and

¹Below this age it is, of course, hard to tell as verbal reports are bound to become less and less detailed as vocabulary and attention span gradually diminish. However, E.R. Jaensch (in Eidetic Imagery (London: Routledge and Kegan Paul, 1930) and Alan Richardson (in Mental Imagery (London: Routledge and Kegan Paul), 1969) argue that very young children, say in the pre-linguistic one to three year old category, are more than likely to have eidetic imagery. Certainly this would go some way to providing an explanation for the small child's frequent confusion of imagined objects and events with reality - the 'scary monster' in the closet conjured up by the three year old is so frightening precisely because it looks to be really present - none of the vividness of the original experience of the monster on television has been lost.

²Psychologists George Humphrey (Thinking an Introduction to its Experimental Psychology, London: Methuen and Co., 1963), Leonard Doob, ("Eidetic Imagery: A Cross-Cultural Will-O-The-Wisp?," Journal of Psychology, 1966); Alan Richardson (Mental Imagery, London: Routledge and Kegan Paul, 1969), F.C. Bartlett (Remembering, Cambridge University Press, 1932); J. Piaget (The Construction of Reality in the Child. New York: Basic Books, 1954); and H. Werner and B. Kaplan (Symbol Formation: An Organismic Development Approach to the Psychology of Language and Experience of Thought. New York: Wiley, 1968) have maintained versions of this hypothesis.

the structure of language imposes a more abstract form upon the way in which our experiences are recorded and stored. Psychologist Alan Richardson states the hypothesis in these terms:

... the trend of these observations provides support for the traditional view that eidetic imagery is part of a more general mode of concrete functioning which in the normally developing Western child disappears as he enters on his high school education. During the pre-adolescent period of physical and cerebral maturation the increased capacity for abstract thought is stimulated and encouraged in most school subjects. In accord with this trend, linguistic skills in oral and written expression take precedence over the inexpressible image. Though some personally experienced events may continue to be registered with something of their original sensory-affective quality, such events are also categorized in more abstract terms. Language is used more and more to compress, to represent and to express our experience ... under these conditions, it is perhaps not so surprising that the ability to use imagery in these who once possessed it begins to wither away from lack of use. Once lost it is not usually regained.¹

Richardson's hypothesis is highly plausible. Language acquisition brings with it a habit of abstraction. With the acquisition of the concept 'red', for instance, we do not acquire a public label for a purely private experience. Rather, we acquire an ability to identify an objective property - a colour property as opposed to a size property or shape property - which is inherent in and characteristic of things as diverse as fire engines, blood, stop lights, and the Canadian maple leaf. Similarly, the small child learns that the term 'teddy bear'

¹Richardson, *ibid.*, p. 40.

applies to pictures in books, Paddington bears on television, and stuffed objects in other children's houses, as well as "Teddy" at the bottom of the bed. And the acquisition of this concept entails the child's recognition of the existence of classes. Teddy is a teddy bear, a member of a class characterized by certain specifiable physical and/or practical properties as well as the (one and only) teddy bear at the bottom of the bed.

In addition to this, the propositional structure of language imposes an abstract form on the way in which our experiences are represented and reported. We are taught to report that we went to the post office yesterday and that we bought so many stamps rather than retrieve in a quasi-sensory affective form a re-experience of the sights, sounds, smells, tastes, pressures and temperatures that were involved in the original experience. And gradually, by a process of social reinforcement, we come to recognize that 'propositional remembering' is generally of more practical use to us than recalling what the sensory-affective experience of being in the post office, buying the stamps, etc. was really like. What we need to know in order to get along in the world and to communicate with other people, are the particular contents of these experiences abstracted and distilled from the sensory context. And language - not imagery - provides a vehicle for this abstraction and distillation.

Assuming, then, that language training has a cumulative effect (facility with language develops in accordance with degree of exposure to it that is), and assuming that the aforementioned facility ensures (or at any rate helps to ensure) the entrenchment of the habit of

abstraction, then it seems entirely reasonable to expect that adults will be less likely to experience imagery than children. They will be likely to recall that such-and-such took place in the morning rather than the various sensory affective states occasioned by observing or indeed by undergoing the event(s) in question. Indeed, it seems reasonable to expect the very gradation in imagery experience among children which the Teasdale and Morsh and Abbott eidetic imagery experiments indicate. Plainly stated, imagery experience should be proportionate to degree of language training and attainment, and the younger a given child is, the less likely he or she is to have been 'trained away' from the sensory and affective to the abstract and conventional. All this means, of course, is that the so-called linguistic theorists - given that they are fully trained language users - are quite likely to be weak imagers.

But beyond this, within the category of 'fully trained language users', there is a further distinction to be made. There are those who have received extra or special language training in the course of their pursuit of a particular field of knowledge. They are, to be more specific, people like the linguistic theorists - philosophers, psychologists, political scientists and mathematicians - who in addition to being conversant in one or more natural language (like English or German) have to come to develop a facility in what might be described as a 'second language'. This is a technical language specifically designed to facilitate discourse in the given academic discipline. Generally speaking, languages of this sort are replete with abstract or theoretical concepts and those who regularly work within the confines

of these languages must be more than usually comfortable with, and indeed given to, abstraction.

We find, for instance, students of politics speaking freely and meaningfully about things like democracy and autocracy even though, strictly speaking, neither of these things can be defined ostensively. They conduct debates and write essays about the human condition, never once referring to an individual human being or to a particular set of circumstances. The focus of the inquiry is an idealized man operating out of an idealized set of circumstances. For the political scientist he is the 'average citizen', the 'man on the Clapham omnibus', or for Marx, the 'exploited worker'. Similarly, the basic objects of concern for the mathematician are numbers, of which there are various kinds - natural, real, infinite, etc. And these are objects which cannot be pointed to. The mathematician working in set theory defines numbers in terms of classes or sets and he is well-aware of the point (made originally by Frege) that having a number (for example, being two), is not a feature of individual things or heaps of things (as being red is) but pertains, rather, to concepts or sets. To a pile of boots as such no number can be attributed. But to the set of boots in the pile or to the concept 'boot in this pile', a number does belong.

Indeed, it would be true to say that, by and large, images would get in the way at the practice of these disciplines. Clearly, the ability to experience vivid visual detail will be of little assistance to the philosopher writing an essay on pornography and censorship. Nor will it be of assistance to the democratic theorist engaged in the process of criticizing the Soviet system of government. The success of

these enterprises depends on the ability of the respective practitioners to abstract from the sensory and affective to the stipulation of what, in principle, constitutes pornography and what constitutes communism or socialism. Being able to call to mind a vivid visual image of a particular scene from a pornographic film or being able to picture Red Square in Moscow during a May Day rally will be of little assistance in this.

My suggestion, then, is this: Given that language training per se seems to have the effect of eroding the phenomenal experience of imagery, it seems reasonable to expect that specialized language training - language training in disciplines with abstract vocabularies and concerns - would be likely to increase the image erosion effect. We should expect, that is, that those who have been trained in the most abstract languages - people like the linguistic theorists for instance - will have the least imagery experience.

Indeed, it could be said that through their extensive language training, the linguistic theorists have been conditioned to represent the world, both publicly and privately (i.e., silently), in abstract linguistic terms. And they are likely to display a marked preference for dealing with the world on a 'hands-off' basis. Since they have very sophisticated language tools available to them - tools which enable them to describe the world in minute detail and to place objects and events in neat conceptual categories - they are likely to attempt to solve problems, even perhaps spatial problems, descriptively and analytically rather than by way of concrete interaction or intervention. Consequently, when they advert to the contents of their so-called

'silent soliloquies' during a problem solving sequence, they regularly find themselves engaged in the construction of propositions and the manipulation of abstract concepts. (See Appendix E.)

Now with specific reference to Shepard's shape assessment problem, we have only one introspective report of an individual who falls into the category of specially trained language user. Philosopher Daniel Dennett reports problem solving without image rotation when he attempts to solve Shepard's problem. He claims that he talks to himself about how the test objects would look relative to one another if one of the objects was to undergo a series of short rotations in the direction of the other. It seems to me that there are two possible explanations for Dennett's report. It could be that Dennett is simply deluding himself as to the nature of his phenomenological experience. This, after all, is what Dennett himself purports to tell other people about their experience. It is possible that Dennett is simply focusing on one stage - perhaps the last stage of his problem solving activity where he registers (linguistically) the outcome of a rotation - such that he fails to remember the former image rotation stage. So Dennett does, or would do, what everyone else does, but is just not attuned to this fact. The second possibility is that Dennett may genuinely not be using image rotation in order to make the required shape assessment. He could be using the linguistic strategy I have discussed earlier. This is a possibility given that Dennett is strongly motivated by his training and his expertise with language to use language wherever he can. If this is the case, then it is a great shame and a sad comment on the utility of philosophical training relative to certain kinds of problem

solving in the real world. For a linguistic strategy, in the case of Shepard's shape assessment problem, is inefficient and, in fact, systematically militates against achieving the solution to the problem.

Between these two possibilities, I favour the former. (There are no data on Dennett's reaction time, and so I cannot argue this definitively as I could if Dennett displayed the linear reaction-time effect.) It may be that philosophers and other specially trained language users do try to solve most if not all of their spatial problems in language, but this is one problem where it seems to me their hand/s will be forced and they are likely to use mental imagery and image rotation - though focusing most particularly on whatever linguistic activity precedes or follows the rotation.

Leaving aside the particular issue of how to interpret Dennett's introspective report, the general point to be taken here is that those who never or rarely experience image-like phenomena in their so-called 'mental lives' are likely to be surprised and even mystified by the suggestion that images and image rotation figure significantly in certain kinds of problem solving. And they are likely to find it difficult to give serious attention to a theory of cognition which gives imagery a central place. As habitual conceptualizers, they are bound to harbour the suspicion (perhaps tacitly rather than in so many words) that there must be something radically wrong here and that what 'people' do - whether they know it or not - is what they do, conduct a kind of silent dialectic in which objects are described, analyzed and inferences are made. Plainly stated, the phenomenological intuition of the

habitual conceptualizer and hence the phenomenological prejudice is that the image theory is wrong and the linguistic theory is right.

Suffice it to say that this phenomenological prejudice - to the extent that it is operative in given philosophers and psychologists - should be resisted. The difference between those who report little imagery and those who report much may be not so much a difference in their actual method of problem solving, but, as I have suggested above, a difference in the habitual modes of reference which the given problem solvers have been accustomed to using when registering that problem solving. In the case of those individuals who have received extensive and specialized language training, it should not be surprising that they are predisposed to reporting and hence predisposed to defending, linguistic/discursive thought.

I suggest then that this phenomenological predisposition or prejudice cannot be dismissed. It may provide (at least) a partial explanation for the out of hand rejection of the image theory which is to be found in the literature. It is perfectly natural that at some stage in their respective analyses the would-be commentators on Shepard's theory would judge that theory by the standard of their own mental lives. And by that standard, the theory is bound to seem incredible. Ask a philosopher when he is engaged in explaining the geometry of triangles, whether his is making (mental) use of an equilateral triangle or a scalene triangle and he will probably evince surprise and state emphatically that he is not using any particular triangle image at all, but the abstract concept of 'triangularity'. Ask a student in an elementary geometry class the same question and he

might well say that he usually prefers to use an isosceles triangle image and he may even go so far as to offer to draw a picture of it (perhaps in its mental colour) for you. From the perspective of the philosopher, this is just plain nonsense. He cannot fathom just how a person would do this or indeed why.

CHAPTER 6

Conclusion: Introspection, The Form of Internal Representation, and Philosophical Prejudice

Doubtless no one would disagree that the first step toward theory construction in any scientific area must be an adequate description of facts, accomplished by means of terms that do not presuppose the theory. What is not so obvious is that there may be alternative frameworks for the description of facts, and the decision made at this choice point may have profound implications for the final theory.

In psychology, it has seemed to many investigators, perhaps a majority, that an adequate descriptive base can be achieved by means of an accurate reading of an organism's observed behaviours, that is, the relationships between stimulus inputs from the environment and the consequent responses elicited from or emitted by the organism. However, in the sphere of cognitive activity, difficulties emerge in that nearly everything that goes on of interest is unobservable. When a person is reading, the investigator can observe and record the movements of the reader's eyes, but he can be under no illusion that he obtains from this record an account of the cognitive activity described as reading. Similarly, in a problem situation, one can observe the environmental context and the way in which it is ultimately changed by the individual solving the problem, but this account provides no information as to what the individual did in arriving at a solution.

In order to proceed toward theoretical accounts of activities such as these, we clearly need factual accounts of what the individual is doing between the initiation of the observed stimulus context and the ultimate observable responses indicating that the individual has obtained information or solved a problem. To be adequate as a basis for theory, this account must be as firmly anchored in observation as descriptions of movements of animals in a maze. How, then, are we to arrive at this objective?

Plain common sense dictates a direct solution to this by way of introspection. It seems reasonable, that is, to expect that an individual engaged in the process of solving a given problem knows or, at any rate, has a good idea of what he is doing. Common sense notwithstanding, however, professionals in the field - philosophers and psychologists - will have none of this. According to them, introspection is not to be trusted. It cannot stand alone as a source of factual data for theories of cognition, indeed, it cannot stand at all.

The first and, I judge, most significant lesson to be learned through the study of Roger Shepard's investigations of the phenomenon of mental image rotation, is that the professional ban on introspection is unjustified. The rotation experiments demonstrate that people can and do solve spatial problems of the given type by means of mental imagery and rotations carried out with that imagery and that they have been correct all along in their declaration of this. Moreover, if we are prepared to suppose that mental rotations are not mental as opposed to physical events but physical events which have a mental dimension in that people experience them, then we have every reason to take people's

introspections of mental rotation quite literally and seriously. For, on such a view, introspective reports of mental rotation amount to observation reports of the actual problem solving activity undertaken.

Beyond this, Shepard's treatment of mental imagery in general breaks an old tradition in philosophy and psychology. Hitherto, those who incorporated imagery into their theories of cognition (people like E.B. Titchener, or John Locke, or David Hume) sought to make it the bedrock of cognitive representation. For them, images constitute the basic elements out of which all thought, not merely memory of things past, is composed.

Shepard, on the other hand, does not conceive the role of imagery on such a grandiose scale. Indeed, he has attempted to study imagery in relation to a single, specific problem task. For Shepard, imagery is a way of representing information that is especially perspicuous for performing spatial cognitions, particularly shape assessments. But no argument is made to justify the claim that all thought processes must involve imagery, nor does Shepard give imagery a privileged position as a form of representation. On the contrary, the information represented in an image is defined vis-à-vis the interpretive procedures that 'read' it as corresponding to an exemplar of some class of entity or activity. That is, if there were no final (linguistic) interpretation of a mental rotation, an image of two objects - one superimposed over the other - would not serve to represent the information that the objects are the same shape.

The lesson to be learned here is that the case for mental imagery is not to be made out on an 'all or nothing' basis - as if images are basic elements of thought or they are nothing at all. Imagery, as Roger Shepard's treatment of it clearly demonstrates, does not have to carry all of the cognitive load. It can assume a more humble role. Even if images are not the elements of thought, they are still important as an 'engineering' feature of the mind-brain, which is not so much necessary as convenient and efficient.

In particular, imagery can function as a computational tool for spatial problem solving. Images allow one to transform information, to mimic dynamic aspects of the environment. Because of the kinds of transformations we can bring to bear and the kind of representation an image is, we can use our imagery as a 'simulation' of possible (and, perhaps, of impossible) transformations in the world. Thus, imagery is an aid to thinking about the consequences of given actions, is a crutch to help us devise a plan for achieving some desired state of affairs.

There is one final lesson to be learned here and this is a lesson for the philosopher of cognition in particular. It is this: The question of the form of cognitive representation is not to be settled, once and for all, on a priori ground - by way of a general pronouncement regarding the nature of representation per se. Experimental results in the field of cognitive psychology require attention and they do occasionally (perhaps frequently) challenge or make obsolete an accepted philosophical principle or rule. Shepard's work on image rotation provides an especially clear demonstration that this can happen. The linear reaction-time pattern exhibited by Shepard's subjects and

their introspective reports stands in contradiction to the widespread philosophical belief that representation can only be linguistic/symbolic in form.

For too long philosophical discussion of mental imagery has been focused on issues of principle rather than empirical fact. The philosopher has been preoccupied with the metaphysical implications of talk of entities inhabiting a mind space. Accordingly, the issue for the philosopher (if, indeed, he acknowledged one at all in recent years) has had to do with whether or not it makes sense to talk (as the casual introspector does) of mental images or whether as Ryle would have it, such talk is born of a fundamental category mistake.

But this is no longer the issue. A case, an empirical case for problem solving by way of images has been made out. Certain behavioural phenomena - reaction-time phenomena occurring as a result of spatial problem solving point clearly and directly to the presence and operation of image-like structures and processes in the human brain. And either the inference from reaction-time behaviour to these structures and processes is reasonable or it is not. In either event, these behavioural facts cannot be ignored by anyone - philosophers included - who presumes to tell us about the nature of thought.

APPENDIX A

Further Reflections on the Common Sense View

It may be wondered whether or not these reflections accurately report the common sense view--so called--on matters of metaphysics. In this connection, I would offer the following points for consideration. In the first place, it seems reasonable to assume that in the common sense context, one of the ways (indeed, perhaps the most significant way) that people develop their ideas about mental activity in general and mental imagery in particular, is on the basis of inferences they might make from their own experience of such phenomena. (They might reason, for instance, thusly: images seem (phenomenally) to be like pictures; pictures are entities, therefore, images are entities too.) Accordingly, I have simply tried to describe, as best I could, my own experience of image-type phenomena, and to describe the sometime paradoxical aspects of this experience, which, in my less reflective, non-philosophical moments, I am simply content to accept at face value. Secondly, the language which people regularly employ in connection with their descriptions of mental imagery--the language of the mind's eye, mental pictures etc.--clearly supports the dualist analysis I have offered here. And, finally, on a more empirical note, I can report that I have gathered some information about the common sense view through discussions with a number of my students.

Specifically, in a seminar which I regularly conduct with a group of students in introductory philosophy, I displayed pictures of several

of Shepard's test object pairs and asked the students to decide whether the objects in each pair are the same three-dimensional shape. When they had done this, I asked them to describe how they solved these problems. Without any prompting from me, they replied without hesitation that they had rotated images 'in their heads'. When I solicited their comments on imagery in general, they avoided explicit definition and concentrated on specific examples describing how and when imagery occurs. When pressed for a definition, they provided an experiential definition. Some said, "Images are things we can see in our minds," and some said, "we just experience them, that's all." The students also seem convinced of the causal efficacy of image experiences, especially in cases like the shape assessment case, where rotations can be carried out with images. When I asked them about brain activity which might be correlated with these image experiences, they were prepared to concede that, of course it is probable that something must happen neurologically to make imagery in the mind possible, but they were not prepared to concede that the brain event and the experiential mental event are identical. One student said (so he apparently believed with significance) that people think with their minds. And some of the students complained they had considerable difficulty conceiving what possible identity there could be between, say, the mind's eye experience of a smooth, continuous expanse of red, as in a vivid sunset, and a brain process that must, at bottom, involve particulate, discontinuous affairs, such as transfers of and interactions among, large numbers of neurons.

APPENDIX B

Brains and Computers

So far as I can tell on my reading of the neurophysiological literature, the consensus among experts is that the brain is, in a number of significant respects, very like a computer. A computing machine is a complex electrical network built up out of a large number of units. The most basic unit is the transistor. There are also units built out of the basic units. These include such components as 'Dynamic Pulse Units'. (A DPU has an input terminal through which it may receive an electrical pulse, and an output terminal through which it may send one), 'and' gates, 'or' gates, and inverters. The brain is also a complex electrical network built up out of a large number of units. The basic unit in the brain is the nerve cell. The behaviour of many nerve cells is something like that of a DPU. If they are stimulated by a nerve impulse from another cell, they send out a nerve impulse of their own. Some nerve cells will act when stimulated by any one of several other cells. These cells behave like 'or' gates. Others will act only when stimulated by all of a group of other cells. These behave like 'and' gates. In these aspects of its behaviour, the brain seems to work like a digital computer.

However, it is possible, too, (and this has been forcefully argued by noted neurophysiologist Karl Pribram)¹ that the action of a nerve

¹Pribram develops his thesis regarding the analogue activity of the brain in Languages of the Brain (Toronto: Prentice-Hall, 1971), and

cell, particularly the duration at the electrical output from the cell, depends on the spatial arrangement of the cells that are joined to it, and the position of the cell that sends it an impulse. In other words, nerve impulses and chemical and physical stimuli to nerve cells seem to add up to a total stimulus. In this respect, the behaviour of the brain is like that of an analogue computer. So the brain, to the extent that it is like a computing machine seems to be one of mixed type, part digital and part analogue.

This is significant, in terms of the postulation of image rotation as a strategy for spatial problem solving for two reasons: First, it tends to support, in a general way, the whole notion of an analogue problem solving activity--in particular, it seems that there is nothing about the nature of the brain which would stand in the way of or make impossible such problem solving. And second, it is very tempting to suppose we have in this analogue activity between/among nerve cells, perhaps the beginning of a mental image, and that a fuller understanding of this will possibly be the key to cashing out Shepard's theory in terms of the neurophysiological hardware of the brain.

more recently, in collaboration with R. Baron and M. Nuwer, in "The Holographic Hypothesis of Memory in Brain Function and Perception," in R.C. Atkinson, H.D. Krantz, and R.C. Lutes, and P. Suppes, (Eds.), Contemporary Developments in Mathematical Psychology (San Francisco: W.H. Freeman, 1974).

APPENDIX C

Evolutionary Considerations

Though Shepard does not make the inference as to the automaticity of the image representation system or the rotation strategy on the reaction-time basis that I have suggested, he does offer the speculation that the system and strategy have probably been pre-programmed in us by a process of biological evolution. Indeed, in the introductory section of their book, Mental Images and Their Transformation, Shepard and Cooper begin by telling us of an observation which Shepard made some years ago: he saw a German Shepherd holding a long stick horizontally in its mouth, run full-speed toward the opening in a picket fence which was large enough for the dog only, to pass through. Apparently just as catastrophe seemed imminent, the dog stopped short, paused for a (thoughtful?) moment and rotated its head and thus the stick through 90° and proceeded through the opening without mishap. Shepard and Cooper argue (presumably on the basis of this and other like observations) that somehow constraints governing the rigid transformation of objects in space have become incorporated into our (as they put it) 'genetically transmitted perceptual wisdom' and function at times automatically. And they offer the a priori justification that surely this is simply a matter of survival. Shepard and Cooper write:

Biological evolution would seem to have embarked upon an uncharacteristically profligate course if the most enduring and pervasive facts about our world--such

as that it is three-dimensional, locally Euclidean, isotropic (except for the unique upright direction conferred by the earth's gravitational field), and populated with objects that therefore have just six degrees of freedom of rigid motion--had not been in any way incorporated into our genetically transmittable perceptual wisdom. Without such incorporation this wisdom, so crucial for survival in such a world, would have to be learned, laboriously and at appreciable risk, by each and every newborn individual *de novo*. (Mental Images and Their Transformation, p. 3).

They continue later in the introduction:

... even if the constraints governing the spatial transformations of objects in the three-dimensional world were first internalized solely as an aid to the perceptual interpretation of external events while those events were actually taking place, the representational machinery embodying these constraints would subsequently have become to some extent autonomous. Except on those occasions when external events become so rapid, captivating, or threatening as to preempt the full resources of the interpretive machinery, that machinery might well be at least partially susceptible to activation from within. Perhaps it is in this way that even in the absence of corresponding external events, we are able to anticipate their concrete outcomes. (Mental Images and Their Transformation, p. 5).

APPENDIX D

The Image Theory of Meaning and Contemporary Empiricism

It is arguable that the modern day empiricist - the sense datum philosopher, so-called, espouses the image theory of meaning as well. Certainly passages in H.H. Price's Thinking and Experience, C.D. Broad's The Mind and Its Place in Nature, and B. Russell's The Analysis of Mind are indicative of this. Russell, for instance, (once) held that ... "the stuff of our mental life ... consists wholly of sensations and images." (The Analysis of Mind, New York, 1921, p. 109). "Thoughts, beliefs, desires, pleasures, pains and emotions are all built up out of sensations and images alone." (Ibid., p. 121). "I think that observation shows us nothing that is not composed of sensations and images." (Ibid., p. 117). And Price speaks of thinking - specifically, problem solving - being accomplished in images and of the importance of resemblance for representation. He writes, ..."it is important to insist that some of us do use images in our thinking. We use visual images rather as we use maps or sketch-plans to find our way about a piece of hilly and wooded country; and when someone asks us the way to Little Puddlecombe, we refer to this mental map and read off the answer, and we can do this because the mental map looks like what it refers to." (Op. cit., 1953, p. 235).

I think that it is important to note, that although the empiricist tradition of images as cognitive representations is certainly carried forward by the sense datum theorists, Price, at any rate, distinguishes

himself from his philosophical predecessors (Locke, Hume and company) by adopting a restrained version of the imagist theory. Price, I take it, does not wish to say that images are the only vehicles of thought - that having the concept of 'Doghood' is necessarily having a picture of a dog in the mind. Rather Price adopts a kind of dual-code theory: "both words and images are used as symbols. They symbolize in quite different ways, and neither sort of symbolization is reducible to or dependent on the other. Images symbolize by resemblance." (Ibid., p. 209). Thus, Price would seem to be somewhat less committed to the privacy implication(s) of the earlier imagist theories.

APPENDIX E

Language Training and Imagery

As far as I know, there are no empirical data which directly support this speculation about the likely correlation between very specialized language training and diminished image experience. No one has undertaken to establish the degree of imagery experience reported by philosophers, say, relative to that reported by carpenters or auto mechanics. There is, however, indirect support for it. For one thing, in the study of imagery experience referred to earlier, Anne Roe found that, among trained academics, there is a gradation in imagery experience. Physical scientists seem to be more given to imagery than social scientists. Since the vocabularies of the various physical sciences are not quite so replete with abstract concepts as the vocabularies of the social sciences (the concept of 'atom', for instance, can be given some kind of concrete reference by way of X-ray photography), it is arguable that Roe's findings are entirely consistent with and indeed predicted by the language training theory.

For another thing, there is evidence which suggests that those who have received language training in languages which are more or less impoverished as far as abstract concepts go, are more likely to retain the image-experience as adults. Leonard Doob (in "Eidetic Imagery: A Cross-Cultural Will-O'-the Wisp," op. cit., 1966) studied the eidetic phenomenon among the members of two societies in Africa - The Ibo of Eastern Nigeria and the Kamba of Central Kenya. These societies pose

interesting test cases because they make use of languages which might be described as 'concrete'. By and large, the Africans seemed to deal with the world on a 'here and now' basis: wherever possible they used ostensive definition to refer to objects and events around them and otherwise displayed a preference for the use of proper names, often times inventing them on the spot, if need be. On testing for eidetic experience among these people, Doob found the age-image correlation to be present. Ibo and Kamba adults were less likely to report imagery than their children. But Doob found one significant discrepancy between the data arising from the Ibo and Kamba tests and data arising from tests of subjects from Western industrialized countries. Notwithstanding the age-image correlation, a goodly proportion of the African adults seemed to have retained their ability to experience images. Doob found that a full 20% of the Ibo adults and 13% of the Kamba adults were given to some form of eideticism. Compared to the less than 1% of non-African adults reporting eidetic experience, this is remarkable and very likely, the explanation for the discrepancy is to be found in the language variable. A number of the Ibo and Kamba adults were able to retain vivid visual imagery because of the relative 'concreteness' of their language.

And finally, though on a more anecdotal note, we find neuro-physiologist D.O. Hebb suggesting a more direct connection between advanced training in the language and concerns of academic disciplines and the erosion of image experience. According to Hebb, the informational content of one's training and the prevailing climate of opinion in the given disciplines may affect the ability to experience imagery.

In the course of his study of the neurological mechanisms operative during so-called 'image sequences', Hebb was forced to reflect critically and philosophically on the notions of introspection and mental imagery respectively. In particular, he considered what it means to talk of pictures in the head and of private, internal seeing, and he came to discover certain conceptual difficulties inherent in this sort of talk. The result of this critical thinking, so he says, was the erosion of his ability to experience images. Hebb writes:

A long time ago I could introspect with ease and did so freely. Becoming more aware that there were theoretical difficulties about introspection, I began to look at the process critically. Eventually, I discovered to my astonishment that it included some imagery of a pair of eyes with the upper part of the face (my eyes and face) somehow embedded in the back of a head (my head) looking forward into the sort of gray cavern Ryle (1949) has talked about. Unfortunately this seemed so ridiculous that I rapidly lost my ability to introspect and now can no longer report on the imagery in detail. But such fantasy in one form or another may be a source of the common conviction that one's mental processes are open to inspection. The imagery is fleeting and unobtrusive and not likely to be reported even to oneself, being so inconsistent with one's ideas of what imagery is and how it works, but it may nonetheless be a significant determinant of thought. (D.O. Hebb, "Concerning Imagery" in Images, Perception and Knowledge. (John M. Nicholas (Ed.), Holland: D. Reidel Publishing Co., 1977), p. 152).

If there is any truth in Hebb's speculation about the connection between his philosophical inquiry into imagery and his ability, subsequently, to experience imagery, then we might well expect that philosophers and psychologists would be the last people likely to

experience imagery. More than any other group of academics, they are likely to have a priori misgivings about the very idea of mental imagery, no doubt inspired, as for Hebb, by the reading of philosophers like G. Ryle.

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